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PROCEEDINGS

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OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

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NOTE.

The object which the Society have in view in publishing their Proceedings is to give an immediate and succint account of the scientific and other business transacted at their meetings to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 7th, 1873.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

Mr. Samuel Broughton was elected Treasurer of the Society in place of the late Mr. Thomas Carrick.

"Atmospheric Refraction and the last rays of the Setting Sun," by DAVID WINSTANLEY, Esq.

It is recorded in the Proceedings of this Society that a letter dated from Southport and written by Dr. Joule was read at the meeting held on the 5th October, 1869. In that letter it is remarked that "Mr. Baxendell noticed the fact that at the moment of the departure of the sun below the horizon the last glimpse is coloured bluish green." Dr. Joule also observes that on two or three occasions he had himself noticed the phenomenon in question and that "just at the upper edge where bands of the sun's disc are separated one after the other by refraction, each band becomes coloured blue just before it vanishes."

During the past eighteen months the writer, from his residence in Blackpool, has had frequent opportunities of observing the setting sun, and has noticed the phenomenon of the final coloured ray certainly more than fifty times. To the naked eye its appearance has generally been that of a green spark of large size and great intensity, very similar to one of the effects seen when the sun shines upon a well cut diamond. The colour however is by no means constant, being often, as in the case of Mr. Baxendell's observation, bluish green, and at times as mentioned by Dr. Joule, quite

blue. The period of its duration too is likewise variable. Sometimes it lasts but half a second, ordinarily perhaps a second and a quarter, and occasionally as much as two seconds and a half.

When examined with the assistance of a telescope it becomes evident that the green ray results at a certain stage of the solar obscuration, for it begins at the points or cusps of the visible segment of the sun, and when the "setting" is nearly complete extends from both cusps to the central space between, where it produces the momentary and intense spark of coloured light visible to the unaided eye.

From the fact of the green cusps being rounded I apprehend that irradiation contributes to the apparent magnitude of what is seen. The range of colour too as seen in the telescope is more varied, and the duration of the whole phenomenon more extended, than when the observation is made only with the naked eye.

Of the objective nature of the phenomenon it is needless to offer evidence; for it needs to be but seldom seen to preclude the idea of an optical illusion. That the waters of the ocean have nothing to do with the production of the colour is made manifest by its visibility when the sun "sets" behind the edge of a well defined cloud. On the 14th and 15th of June, for instance, it was seen at upper contact of the solar limb with clouds. On the earlier date in question a thin band of cloud stretched across the setting sun, and under a power of fifteen diameters the green effect was seen at upper contact with the cloud and again at final disappearance below the horizon. On the later date it was again seen at upper contact with each of several filaments of cloud and again at final disappearance. And on several other occasions the writer has observed the effect when the disappearance of the sun has taken place at an elevation of six or eight degrees behind a heavy bank of clouds.

Respecting the increased range of colours seen when the

phenomenon is observed with telescopic aid, I may mention that on the 28th of June the sea was calm and the sky quite cloudless at the setting of the sun. Of the final coloured rays fifteen diameters showed the first to be a full and splendid yellow, which was speedily followed by the usual green, and then for a second and a half by a full and perfect blue. Respecting the increased duration of the colour I have found that when the atmosphere is sufficiently favourable to allow a power of 60 diameters being employed with a three-inch object glass, the green effect is seen at that part of the sun's limb in contact with the horizon even when one half the sun is still unset, and of course from then till final disappearance.

The different colours seen, together with the order of their appearance, are suggestive of the prismatic action of the atmosphere as the cause of their production, and the interception of the horizon or the cloud as the cause of their separation.

Assuming the correctness of this view, it becomes evident that an artificial horizon would prove equally efficacious in separating the coloured bands, and also that if employed during an inspection of the sun's lower limb, the least refrangible end of the spectrum would be disclosed. Accordingly I introduced into an eyepiece of my telescope a blackened disc of metallic copper, having a slit cut in it of about the one hundred and fiftieth of an inch in width, and proceeded to make an observation in July when the sun was about one half of its meridian height. The blinding glare however of that portion of the sun seen through the slit rendered the observation futile. By projecting a large image of the sun into a darkened room I was enabled to get the whole of the spectrum produced by the prismatic action of the atmosphere in a very satisfactory manner. In this case a semicircular diaphram was used, so placed that its straight edge divided the field of view into equal parts, from

one of which it obscured the light. The diaphram was placed as before in the focus of the eyepiece, and by rotating it every portion of the sun's limb could be in turn examined, and that too in the centre of the field, so as to be equally subjected to the minimum of the peculiarities of the instrument. When the sun's lower limb was allowed to descend into the field of view the first rays were intensely red. After a momentary duration they gave place in succession to orange, yellow, and green, which were then lost in the ordinary refulgence of the sun. The upper limb gave green, blue, and finally purple, which latter colour I have thus far never seen upon the natural horizon. It should be remarked that the colours seen were vivid and unmistakeable, and each one of them easily detained at will or the whole phenomenon recalled by the adjusting screws of the instrument. I apprehend that the results here given sufficiently prove that atmospheric refraction is the cause of the coloured rays seen at the moment of the sun's departure below the horizon. I have however thought it worth while to examine the light proceeding from the moon's limb by the aid of the artificial horizon and of course by direct observation. The results were decisive and satisfactory, the spectral colours being easily observed. The green effect I have also frequently seen on the departure of the moon beneath the edge of a dark and well defined bank of clouds. Telescopic aid has however in every instance been required.

The rapid changes in colour observable in the case of almost any large fixed star at an elevation of twenty or thirty degrees above the horizon, and which changes vary between red, green, and blue, may I think be fairly attributed to the same cause as the colour in the sun's final ray. Particles of dust floating in the air act, I apprehend, for the moment in the capacity of diaphram or horizon, and thus enable the eye to perceive even in the light of the stars the prismatic action of our atmosphere.

Ordinary Meeting, October 21st, 1873.

EDWARD SCHUNCK, Ph.D., F.R.S., F.C.S., Vice-President, in the Chair.

W. BOYD DAWKINS, F.R.S., exhibited a fragment of a post struck by lightning on 2nd June, 1873. It formed one of three about 8 feet high and 15 feet apart in the garden of 11, Norma Road, Rusholme, and stood under a cherry tree of which the stem was 10 feet away. It was completely shattered, fragments being driven as far as the walls of the house, 25 yards off, and the downward direction of the loose splinters implied that the explosive force was exerted from below upwards, instead of from above downwards. People in the Dickenson Road observed what they termed a "thunderbolt" fall, as they thought, on the house, and some of the inhabitants describe it as a flame of light followed immediately by a crash of thunder. It is very probable that the explosion was produced by an electric current passing from the earth upwards, and not *vice versa.*

Professor REYNOLDS attributed the shattering of the post to the explosive or repulsive action of an electrical discharge of unusual intensity.

Mr. BAXENDELL thought it was most probably due to the sudden conversion of a portion of the moisture in the post into steam of high tension by the heating action of the electrical discharge, and mentioned instances in which condensed vapour was said to have been seen rising from trees immediately after they had been struck by lightning.

"On the Relative Work spent in Friction in giving Rotation to Shot from Guns rifled with an Increasing, and a Uniform Twist," by OSBORNE REYNOLDS, M.A., Professor of Engineering, Owens College, Manchester, and Fellow of Queens' College, Cambridge.

The object of this paper is to show that the friction between the studs and the grooves necessary to give rotation to the shot *consumes more work with an increasing*

than with a uniform twist; and that in the case of grooves which develope into parabolas, such as those used in the Woolwich guns, the waste from this cause is double what it would be if the twist was uniform. I am not aware that this fact has ever been noticed. It must not be confounded with the questions already at issue respecting the Woolwich or French system of rifling guns. The advocates of the gradually increasing twist maintain that it relieves the pressure between the studs and the grooves at the breech of the gun, where it would otherwise be greatest, while the opponents argue that in order to obtain this otherwise advantageous result, the bearing surface of the studs has to be so much reduced that they are not so well able to withstand the reduced pressure as they are to withstand the full pressure with the plane grooves. Now I bring forward a collateral point, which has no bearing on the previous question, but which is, in itself, of sufficient importance to influence the decision in favour of one or other of these systems. I show that apart from any undue wedging or shearing of the studs, that with nothing but the legitimate friction, the amount of work wasted in imparting rotation to the shot is nearly twice as great with the parabolic as with the plane grooves. This is important, for, although the magnitude of this waste does not appear as yet to have been the subject of direct inquiry, it will be seen from what follows that with the plane grooves it amounts to more than one per cent of the whole energy of the shot, and, consequently, with the parabolic grooves it will amount to two per cent of the energy of the shot; this is, to say the least, important as regards the effect of the discharge; and when we consider that all the work spent in friction is spent in destroying the gun and the shot, we see that it becomes a matter of the very greatest importance whether the gun spends one or two per cent of its power on self-destruction. It was established as a fact in the trials of 1863–5, that the guns with an increasing twist gave a lower velocity than those with the uniform twist. In the trial with the two seven-inch guns made especially to test this point, the dif-

ference of velocity was such as to make three per cent difference in the energy of discharge — a result somewhat greater than what would have been due to the legitimate friction, unless the coefficient of friction between the studs and the grooves was excessively high from some cause, such as the cutting of the studs into the grooves. However, it would seem that the conclusions at which I have arrived are in accordance with actual experience, and help to explain what was otherwise to a certain extent anomalous.

Although these conclusions cannot be definitely proved without the aid of mathematics, they may be shown to be true (or reasonable) under certain circumstances, as follows:

The work spent in friction will, both with the parabolic and plane grooves, be equal to the coefficient of friction multiplied by the mean pressure on the studs and again by the length of the grooves (or by the length of the gun—nearly). Now, the coefficient of friction and the length of the gun are the same in both cases; hence this work will be proportional to the mean pressure on the grooves throughout the gun. Again, if the pressure on the parabolic grooves is constant (which it is the object of these grooves to make it), then the mean pressure in both cases will be inversely proportional to the angle which the shot turns through while in the gun. This follows directly from the fact that the speed and consequently the energy of rotation with which the shot leaves the gun is the same in both cases; for this energy is nearly equal to the mean pressure multiplied by the arc through which the studs turn,* and hence the mean pressure is equal to the energy divided by the arc.

We have then the work spent in friction proportional to the mean pressure; and the mean pressure inversely proportional to the angle turned through by the shot in the gun; therefore *the work spent in friction is inversely proportional to the angle turned through by the shot in the gun.*

Now, the angle turned through with parabolic grooves is

* This is always true for plane grooves, but it will only be true for parabolic grooves when the pressure on the studs is constant all along the grooves.

half the angle turned through with plane grooves (by a property of the parabola); hence the work spent in friction with the parabolic grooves is double what it is with the plane grooves. This may be shown mathematically as follows :—

I. *To estimate the actual work spent in friction with plane grooves.*

Let μ = coefficient of friction.
i = the inclination of the grooves.
K = work spent in friction.
Σ = the energy of discharge or the striking force with which the shot leaves the gun.

$$\text{Then, } K = \frac{\mu i}{2} \times \Sigma.$$

For if R = the mean pressure on the grooves, l = the length of the gun, then $K = \mu R l \sqrt{i^2 + 1}$ . . . . (1)

$$\text{And the energy of rotation} = \frac{i^2}{2}\Sigma = \frac{R l i}{\sqrt{i^2+1}}.$$

$$\therefore \frac{i^2}{2}\Sigma = \frac{R}{\sqrt{i^2+1}} l i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\therefore K = \frac{\mu i}{2}\Sigma.$$

Hence (with a gun making one turn in 35 diameters) where $i = \frac{1}{11}$ and $\mu = \cdot 3$.

$$K = \frac{\cdot 3\,\Sigma}{22} = \cdot 013\,\Sigma.$$

The equation $K = \frac{\mu i}{2}\Sigma$ shows, what is otherwise quite obvious, that with the plane grooves the work spent in friction is independent of the distribution of the pressure within the gun, and is proportional only to the energy of discharge; and hence will be the same, whether the powder is quick or slow, provided the shot leave with the same velocities.

This, however, is not the case with the parabolic grooves. It is obvious that the friction will involve the law of pressure in the gun. Consequently, we cannot calculate this work unless we make some assumption with regard to the law of pressure.

II. *To estimate the actual work spent in friction with parabolic grooves when the pressure on the studs is constant.*

Let $x = \frac{y^2}{b}$ be the equation to the developed grooves, and let s be the length of the grooves. Then, if we assume that $\frac{dy}{ds} = 1$, and that K_b (the work spent in friction with the parobolic grooves) $1 = \mu R l$, we have the work of rotation

$$= \int R \frac{dx}{dy} dy$$

$$= \frac{l}{2 b \mu} K_b$$

And the work of rotation $= \frac{i^2 \Sigma}{2}$

$$\text{Since } i = \frac{l}{b},$$

$$\therefore K_b = \mu \, i \, \Sigma,$$

And for plane grooves $K = \frac{\mu \, i \, \Sigma}{2}$

$$\therefore \frac{K b}{K} = 2.$$

An expression for this work might have been obtained without assuming $\frac{dy}{ds} = 1$, but so long as i is less than $\frac{1}{10}$ the difference is very small.

Hence we see that on this assumption the work spent in friction with the parabolic grooves is twice as great as with the plane grooves. This assumption is not an unreasonable one, for the declared object of the increasing twist is that it may equalise the pressure of the studs on the grooves throughout the gun. However, it is not to be supposed that this object is always attained, for one kind of powder has a different law of force from another. It is necessary therefore to consider other laws of force. We cannot obtain a general expression which will include all, but we may examine several laws of force which will enable us to see how far the law of force affects the results.

In all cases the force diminishes from the breech to the muzzle, and the law may be roughly expressed by $P = \frac{\lambda}{a + y}$

where y is the distance of the shot from the breech, and a and λ are constants for each class of powder.

Although with this value of P the equations of motion cannot be solved rigorously, an approximate solution may be found as follows:—

III. *To find the ratio of work spent in friction with parabolic and plane grooves when the law of force is*

$$P = \frac{\lambda}{a+y}.$$

The equations of motion are

$$\frac{1}{2}\frac{d}{ds}(v^2) = P\frac{dy}{ds} - \mu R \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$R = P\frac{dx}{ds} + \frac{v^2}{\rho} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Neglecting the μ R as small in (1), and taking ρ (the radius of curvature) $= b$, we have if $\frac{dy}{ds} = 1$

$$\frac{v^2}{2} = \int P\,dy = \lambda \log. \frac{a+y}{a}.$$

$$\int R\,dy = \int P\frac{y}{b}dy + \frac{2\lambda}{b}\log.\frac{a+y}{a}$$

$$\text{or } K_b = \mu\int R\,dy = \frac{\mu\lambda}{b}\left\{ (2l+a)\log.\frac{a+l}{a} - l\right\}$$

And for plane grooves ρ is infinite and $\frac{dx}{dy} = i$

$$\therefore K = \mu\int R\,dy = \mu\lambda i \log.\frac{l+a}{a}$$

$$\therefore \frac{K_b}{K} = 2 + \frac{a}{l} - \frac{1}{\log.\left(1+\frac{l}{a}\right)}$$

If $a = 0$ so that $P = \frac{\lambda}{y}$

$$\frac{K_b}{K} = 2.$$

If a is very great, so that $P = \frac{\lambda}{a}$

$$\frac{K_b}{K} = \frac{3}{2}$$

And the former law more nearly expresses the condition of most guns.

IV. *If we take a law of force.*

$$P = \frac{e^{-2\mu \tan.^{-1}\frac{y}{b}}}{b^2 + y^2}$$

the equations of motion can be solved.

And if $i = \frac{l}{b}$, $\theta = \tan.^{-1} i$.

We get $K_b = \frac{e^{-2\mu\theta}}{2\mu b}\left\{e^{2\mu\theta} - 1 - 2\mu\theta + \mu^2\log.(1+i^2)\right\}$

And for the plane curve $K = \frac{i}{2b} e^{-2\mu\theta}\left\{e^{2\mu\theta} - 1\right\}$

And therefore $\frac{K_b}{K} = \frac{e^{2\mu\theta} - 1 - 2\mu\theta + \mu^2 \log.(1+i^2)}{i\left(e^{2\mu\theta} - 1\right)}$

From which, for any given value of θ, we may obtain the actual value of $\frac{K_b}{K}$.

When θ is small, so that where we may neglect high powers without error $\frac{K_b}{K} = \frac{3}{2}$.

Which result is in exact accordance with those previously obtained, for, it must be noticed, that with this law P is nearly constant. Hence we arrive at the following conclusions:—

(1) That when the pressure of the powder is constant,

$$\frac{\text{Work spent in friction with parabolic grooves}}{\text{Work spent in friction with plane grooves.....}} = \frac{3}{2}$$

(2) That when the pressure diminishes rapidly the above ratio = 2.

(3) That this ratio may have any values between these two, but that it cannot go beyond these limits.

Mr. BAXENDELL read the following extract from a letter he had received from the PRESIDENT:—

You will see that I have put a little drying apparatus to the short limb of my syphon barometer. I believe that a long open tube attached to the short end by a bit of india-

rubber tube will do just as well. This I am going to try, and also to exclude the air more perfectly than I find it is in the instrument at the Rooms. The principal fear was that the sulphuric acid would slowly act on the mercury. I think the barometer has been put up long enough to decide this, and I feel convinced that the plan will succeed.

Ordinary Meeting, November 4th, 1873.

R. ANGUS SMITH, Ph.D., F.R.S., &c.. Vice-President, in the Chair.

Mr. Joseph R. Bridson, Mr. James Watkins, and the Rev. William Marshall, B.A., were elected Ordinary Members of the Society.

The CHAIRMAN said that the death of Dr. Calvert, one of the most distinguished members of the Society, called for more attention than he was able to give it, but this meeting of the society could not be allowed to pass over without a few words regarding the loss which all chemists must feel. It will no doubt be the pleasant duty of some of his friends to prepare a more detailed account of his labours. Meantime he (the Chairman) would express the opinion of all who knew Dr. Calvert by saying that a more diligent student of chemistry has rarely if ever been found in any country. It has been remarked that Dr. Calvert's knowledge of the literature of science was something marvellous. This was doubtless owing to his devotion to the subject and his untiring activity and strength. The memoirs which he has written are too numerous to be characterized at present, and already several journals have given the heads of the most important. As a medium of communication between scientific men, manufacturing and professional, in France and England, he was no doubt the means of doing much good in both countries. In the former country he felt almost as much at home as in the latter, having lived there from the time he was fourteen years of age until he was twenty-eight,

and having married a French lady. It was this intimate connection with France which gave him his French accent, which he could never entirely get rid of. He was however entirely English by birth. His habits contracted there may have led also to that excessive activity which seems to have told at last in a very unexpected manner upon his health, as he seemed powerful and of sound constitution. The fatigues he underwent during a visit to the Vienna Exhibition, and the climate there, must, however, be blamed, so far as we can hear, for the evils both direct and indirect which produced a fatal result.

Dr. Calvert had, shortly before his death, completed a revised edition of his lectures on some departments of manufacturing chemistry. His practical experience, combined with his profound knowledge of the theory of the subject, must render such a work of great value, and keep his name for a long time fresh among chemists.

His later investigations, especially those connected with germs or the beginnings of life, were of a purely scientific character.

He may be said in every sense to have been a successful man until that illness which destroyed a life which promised to be very long. He had many friends, and amidst an excessive amount of work he was able to be obliging and kind to a large number.

He was a Fellow of the Royal and the Chemical Societies and of several foreign Academies, and in the scientific circles of London and Paris he was as well known as in his adopted city, Manchester, where many lament him as a man who knew little of him as a chemist.

"On the Bursting of Trees and Objects struck by Lightning," by Professor OSBORNE REYNOLDS, M.A.

The results of the experimeuts referred to in this Paper were exhibited to the meeting.

The suggestion thrown out by Mr. Baxendell at our last meeting—that the explosive effect of lightning is due to the conversion of moisture into steam—seemed to me to be so very probable that I was induced to try if I could not produce a similar effect experimentally.

1. I first of all tried to burst a thin slip of wood by discharging a jar through it, taking care so to arrange the wood that the discharge should be of the nature of a spark and not a continuous discharge; this was done by making the wood to form part of a discharging rod with balls on the ends.

This experiment was successful in the first attempt, although the results were on a small scale.

It should be mentioned that the wood had been damped with water.

This experiment was repeated with larger pieces of wood with various results.

2. It then occurred to me to try with a glass tube. This I did at first with a very small tube, passing wires from the ends of the tube until they were within $\frac{1}{4}$ inch of each other.

The small tubes burst both with and without water.

3. I then used a larger tube (about $\frac{1}{16}$ inch bore), using it in a similar manner. The discharge without water produced no effect on this, even when repeated several times, but when the tube was full of water (with the ends open) the first discharge shattered that part of the tube opposite the gap in the wire. This tube was bent in the form of a syphon, and the water stood about 1 inch beyond the gap in the wire on each side of it.

4. I then tried a stronger tube which I had been using for insulation. It had a bore of $\frac{1}{8}$ inch and was $\frac{3}{8}$ inch in external diameter. It was capable of sustaining a pressure of probably 10,000, and certainly 5,000 lbs. on the square inch, that is to say, a pressure of from 2 to 5 tons

per square inch. It was about 14 inches long and bent in the form of a square-ended siphon. The gap in the wire was about ⅛ inch, and the water extended about 1½ inches on each side of the gap. The ends of the pipe were open, and the jar charged in the same manner as before with about 100 turns of a 12 inch plate machine. The surface of the jar is about ½ a square foot, and the discharge when effected with the common rod took place through about 2 inches of air.

This tube was shivered at the first discharge. That part opposite the gap and for some way beyond is completely broken up into fragments which present more the appearance of having been crushed by a hammer than of being the fragments of a pipe burst under pressure. Some of the fragments show that the interior of the pipe has been reduced to powder.

These fragments were scattered to some feet on all sides, but there was nothing like an explosion. I held the pipe in my hand at the time of the discharges, and the sensation was that of a dead blow. There was no noise beyond the ordinary crack of the discharge.

The manner in which this pipe was destroyed clearly showed that a larger one might have been broken. But as it was two o'clock and my fire was out, I did not continue the experiments.

It is not easy to conceive the precise way in which a pressure of probably more than 1,000 atmospheres could be produced and transmitted in a pipe of water the ends of which were open. It might have been caused by the sudden formation of a very minute quantity of steam, or by the expansion of the water; but whichever way it was, its effect was due to its instantaneous character, otherwise there would have been an explosion,

When we consider the great strength of this pipe (which might have been used for a gun without bursting), and when

we see that it was not only burst but that the interior of the glass was actually crushed by the pressure, and all this by the discharge of one small jar, we must cease to wonder at the bursting power of a discharge from the clouds.

The Rev. W. N. MOLESWORTH, M.A., said, that in answer to an appeal that had been made to him by the chairman, he would bring under the notice of the society some Roman and Celtic antiquities, to which he thought that sufficient attention had not been given in this country.

1. He believed that few people in Manchester were aware that on the side of Blackstone Edge, and within a little more than fourteen miles from their city, there was a Roman road, which was one of the most perfect and remarkable that existed anywhere in the world. A thin coating of earth and heather had so completely preserved it that in parts it was as fresh and new in its appearance as if it had been quite recently constructed. It was composed of two distinct ways, each just broad enough for a rather narrow cart to pass along it, so that it would seem that in the construction of this road our railway system had been anticipated, and that there was what may be called a double line, one for ascending and the other for descending carriages. These two ways were separated by stones nearly a yard wide, and in which a deep groove was cut, apparently designed for an aqueduct. At the top there was what was described in the ordnance map as a fort, but what he believed to be merely the quarry from which the stone used in the construction of the road had been obtained. Mr. Molesworth stated that he had inquired of many antiquarians both in this country and in France, but had not been able to meet with any one who had seen a Roman road that resembled that which he had described.

2. The next monument to which he wished to call attention was the Cité de Limes, or de Lenies, otherwise called

the Camp de César, about two miles to the north east of Dieppe. It is a vast Celtic camp of triangular form, capable of containing a population larger than that of the city of Manchester. It is bounded on one side by inaccessible cliffs of great height; on the second side, where it is partly defended by nature, with an earthen wall about 36 feet high; and on the third side, which is less defended by nature, with an earthen wall about 56 feet high, and as thick as a railway embankment. The two walls together are probably about three miles in length. They are supposed to have been constructed by the Celts, and the space they enclosed to have served as a place of refuge for the inhabitants of the neighbouring country, who, when invaded, probably fled into, taking with them their families and all their moveable possessions. Mr. M. stated that he had seen a great number of flint implements that had been found within the enclosure. There is a very good model of it in the museum of Dieppe. There are two similar monuments in Normandy: one at Caudebec, on the Seine. It is in the form of a very long isoceles triangle, the two sides of which are inaccessible cliffs, and the base protected by a wall similar to that just described.

3. Mr. Molesworth also referred to the vitrified camp which exists in the neighbourhood of St. Brieux in Brittany. This camp is enclosed by a wall of granite, which has been completely calcined by the action of fire. It was supposed by the Archæological section of the Congrès Scientifique of France who visited it that it had been occupied by persons whose conduct had rendered them so obnoxious to the inhabitants of the country that they were determined to destroy their camp so completely as to prevent it from affording them shelter at any future time. It was very extraordinary that in these times in which the fort was thus burnt heat could have been obtained sufficiently intense to destroy the nature of the granite. In answer to an inquiry as to

whether the effects described might not have been produced by long exposure, Mr. M. stated that he believed that the French archæologists who visited it were unanimously of opinion that it had been acted on by fire. There were also some vitrified camps of a similar character in the north of Scotland.

Dr. ANGUS SMITH said that he could certainly confirm from personal observation the opinion that Mr. Molesworth had given, by saying that vitrified forts generally, and probably always, are the result of premeditated firing. He had seen none in France, but their chief seat was in Scotland, where there were many. He had spent some time in examining Dan Macuisneachan or Macsniochan in Argyleshire, and having seen the vitrified matter resting on the unaltered rock below, there was no room for belief that internal fire, to which some persons attributed the combustion, had ever interfered. Although Mr. Molesworth had not seen actual vitrified matter at the fort he visited at St. Brieux, but had found only granite which had evidently been acted on by fire, he (Dr. S.) had specimens where the vitreous matter had evidently been ready to drop at the moment of its cooling and congealing. Many tons of vitrified matter may be seen on the Top of Noath, north of Aberdeen, and on Knockfarrel, near Strathpeffer, and indeed in many places. On one in Hungary, described by Dr. Stuart of Edinburgh and others, the layers of charred wood are seen alternating with the stones. None of this kind were known to Dr. Smith as being in Scotland. He had analysed the vitrified matter of a small fort in Bute, and one on West Loch Tarbert, Argyle, and found an increase of bases over the matter of the stones not vitrified. The ashes of the combustible would no doubt help to vitrify the surfaces and so cause adhesion, whilst the heat which penetrated the whole mass often broke up even sandstones, and bent or made brittle almost every species of the mixed rocks used. There seems

however to have been an inclination to have some basaltic rocks to assist fusion. The vitrified walls are often very extensive and the plan is systematic; no conceivable siege fires or other violent and fitful work would produce the result. But as he (the speaker) had written three papers more or less bearing on this subject, he need not say more.

Ordinary Meeting, November 18th, 1873.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

Mr. Arthur William Waters, F.G.S., Professor Arthur Gamgee, F.R.S., and Mr. Arthur Schuster, Ph.D., were elected Ordinary Members of the Society.

"On the Bursting of Trees and Objects struck by Lightning," by Professor OSBORNE REYNOLDS, M.A.

In a paper on this subject read at the last meeting of this society I stated that the tube which was burst by a discharge from a jar would probably withstand an internal pressure of from 2 to 5 tons on the square inch; and I made use of the expression the tube might be fired like a gun without bursting. These statements were based on the calculated strength of the tube, and with a view to show that there was no mistake, I have since tried it in the following manner.

I made 3 guns of the same tube. No. 1, which was 6 inches long, had its end stopped with a brass plug containing the fuze hole. No. 2 and No. 3 were 5 inches long and had their breeches drawn down so as only to leave a fuze hole. These tubes were loaded with gun-powder and shotted with slugs of wire which fitted them, and which were all $\frac{3}{4}$ inch long.

No. 1 was first fired with $\frac{1}{2}$ inch of powder, the shot penetrated $\frac{1}{4}$ inch into a deal board, and the gun was uninjured.

No. 2 was then fired with $1\frac{1}{2}$ inches of powder, and the shot went through the 1 inch deal board and $\frac{1}{2}$ inch into some mahogany behind, thus penetrating altogether $1\frac{1}{2}$ inches; the tube, however, was burst to fragments. Some of these were recovered, and although they were small they

did not show cracks and signs of crushing like those from the electrical fracture.

No. 3 was then fired with ¾ inch of powder, and the shot penetrated ½ inch into the deal board.

It was again fired with 1 inch of powder, and the shot penetrated 1 inch into the deal.

Again it was a third time fired with 1¼ inches of powder, when it burst and the shot only just dented the wood.

These experiments seem to me to prove conclusively the great strength of the tube and the enormous bursting force of the electrical discharge.

"On the Colour of Nankin Cotton," by EDWARD SCHUNCK, Ph.D., F.R.S.

Among the numerous varieties of cotton existing in commerce there is one which cannot fail to strike the most unpractised eye, in consequence of the peculiar colour, varying from a pale yellow or rather fawn to a brown or reddish-brown, which it exhibits. This kind of cotton is generally called "Nankin" cotton in consequence of its having been used at an early period by the Chinese for the manufacture of the fabric called nankin or nankeen, the peculiar colour of which is so well known as to need no description. Specimens of raw cotton of the colour referred to from other countries, such as India, America, the West Coast of Africa, and the shores of the Mediterranean are, however, found in all extensive collections, so that it cannot be considered as a product peculiar to China. In Malta it is, I am informed, especially abundant, more so than the ordinary white kind. Whether it is produced by a peculiar variety (not to say species) of the cotton plant or whether the colour is owing to peculiarities of climate, soil, or method of culture influencing the plant is, on the other hand, a question not easily determined. Considerable doubt indeed prevails as to the number of species embraced by the genus Gossypium

and the characters by which they are distinguished from one another, some authorities admitting only four species, whilst others describe more than twenty. Among the former is Dr. Forbes Royle, who says*: "The result of our investigation of the species of the genus Gossypium is, that there are at least four distinct species which may be easily distinguised, and that the great mass, probably the whole of the cotton of commerce, is yielded by three of these species and their varieties." Attempts have been made to distinguish the various species of Gossypium according to the colour of the cotton produced by them, but as might be anticipated with little success, since the colour of the organs of plants seems to be one of the least persistent of their characteristics. Anyone, indeed, examining a collection of specimens of cotton fibre must see that there are few marked distinctions between them as regards colour, none being absolutely white, and the greater number exhibiting various shades of cream colour, verging to fawn. Nankin cotton may be considered as being placed, as far as colour is concerned, at the extreme end of the scale, at the other end of which we find Sea Island and other almost pure white kinds. Several authorities assert, it is true, that Nankin cotton is produced by one species of the genus only, viz., *G. religiosum*, but others say it is found on more than one species. Among the latter I may again quote Dr. Forbes Royle, who says: "*G. religiosum* of Linnæus seems to be distinguished from other species only by having tawny-coloured cotton; but we have seen that both the common Indian cotton, the Chinese cotton, the arboreous species, and *G. barbadense* all occasionally produce nankeen-coloured cotton, and that; therefore, it cannot be considered as characteristic." Referring to "China cotton" the same author says†: "The specimen in *Herb. Hook.*, from Mr. Fortune, is

* On the Culture and Commerce of Cotton, &c., p. 151.

† On the Culture and Commerce of Cotton, &c., p. 143.

less hairy than most Indian specimens, though clothed with a number of short hairs. Mr. Fortune states, in a note with some specimens that he sent to Dr. Lindley, from China, that the white-coloured and the nankeen-coloured cotton are yielded by the same species and even by the same plant, and that the two kinds are separated by the Chinese. Besides India and China, this species is cultivated in Persia, Syria, Asia Minor, and the Islands of the Mediterranean, as well as in the north of Africa and the south of Europe. The kind yielding the nankeen-coloured cotton in Malta is probably a variety." Fortune, in his Travels,* makes the following statement regarding the cotton plant of China: "The Chinese or Nanking cotton plant is the *Gossypium herbaceum* of botanists, and the '*Mie wha*' of the northern Chinese. It is a branching annual, growing from one to three or four feet in height, according to the richness of the soil, and flowering from August to October. . . . . The yellow cotton, from which the beautiful Nanking cloth is manufactured, is called '*Tze mie wha*' by the Chinese, and differs but slightly in its structure and general appearance from the kind just noticed. I have often compared them in the cotton fields where they were growing, and although the yellow variety has a more stunted habit than the other, it has no characters which constitute a distinct species. It is merely an accidental variety, and although its seeds may generally produce the same kind, they doubtless frequently yield the white variety, and *vice versâ*. Hence specimens of the yellow cotton are frequently found growing amongst the white in the immediate vicinity of Shanghai; and again a few miles northward, in the fields near the city of *Poushan*, on the banks of the Yang-tse-Kiang, where the yellow cotton abounds, I have often gathered specimens of the white variety." The opinion here expressed is confirmed by Parlatore,† who affirms,

* Two Visits to the Tea Countries of China, vol. I., p. 199.

† Le Specie dei Cotoni. Firenze, 1866.

without hesitation, that the plant bearing red or reddish cotton is merely a variety either of *G. arboreum* or *G. hirsutum*.

Mr. Thomas Clegg, of this city, who is familiar with the properties of the various kinds of cotton and well acquainted with their commercial value and places of growth, has kindly lent me some of his specimens for exhibition on this occasion, and in a communication received from him he has given me some information regarding Nankin cotton which will no doubt be of interest to the meeting.

Mr. Clegg says: "I found Nankin cotton abundantly at Malta, many parts of Tunis, and in great quantities on the West Coast of Africa. Dr. Livingstone has sent me many samples of it, and I have frequently had specimens of it from other, but always arid, dry, and hot parts of the world. The Maltese has however always been the best. It is very short in staple, coarse and of little value in itself, especially as so little of it is produced. Being high coloured, of course if used alone it would give a high peculiar colour to the cloth, and as mixing it with whiter cotton generally stripes and spoils the cloth, it is in very bad repute. In China and Japan it gets more dusky and dark and even lower in staple. On the West Coast of Africa it seems to be hybridized and modified in colour. The seed, when cleaned from the cotton, is generally only half clothed with the fibre, the other half being black. But whether entirely Nankin-coloured or a little whiter, it is always on that coast much longer in staple, and though rather coarse, still a good useful cotton. Indeed the generality of the West Coast Cotton is a nice cream-coloured cotton, a little higher than the old Demerara cotton used to be, and of staple on an average fully equal to American bowed upland, or the lower class of New Orleans, and I hardly think can be classed as Nankin at all, though high-coloured. If it could be had in quantity it would, in my opinion, soon supersede all the lower qualities of American

cotton. Nankin cotton is always, so far as I have seen, from fibrous-coated seed. As to the colour, I cannot tell, being no chemist, whether it is fast or not, but it never seems to fade with me. . . . . According to my experience it is only in very hot countries, and on a rather arid soil, that the really dark Nankin is produced. I think if experiments were tried for three or four years together, Maltese on the West Coast of Africa would resemble African, and West African seed sown at Malta would become Maltese cotton; and I almost think that West African seed which at home produces yellow-tinged cotton, would, in one or two years, in the New Orleans district, produce white cotton. And I further think that pure New Orleans seed sown at Malta in three or four years would give cotton of the red tinge of ordinary Maltese." Mr. Clegg seems therefore to agree with those who think that the variations in colour observed in cotton are entirely owing to differences of climate and soil, and are not peculiarities attaching to different species of the plant.

These remarks will suffice to give a general idea of the properties of Nankin cotton and its supposed origin. I propose in this communication to give a short account of some experiments made to ascertain the cause of the peculiar colour by which it is characterized.

The colour of Nankin cloth having been successfully imitated in this country by depositing oxide of iron on and within the fibres in the manner well known to dyers, it might be supposed that the colour of the raw cotton was due to iron in some form. The simplest experiments prove this however not to be the case. The cotton on being incinerated leaves an ash which does not contain a larger proportion of iron than that of ordinary cotton, and the colour is not removed by treating the cotton with dilute mineral acids capable of dissolving oxide of iron, whereas the colouring matter dissolves, though slowly, on boiling it

with caustic soda lye. Another mode of imitating the colour consists in mordanting the fabric with alum and then dyeing with oak bark, the process resembling that by which calico is ordinarily dyed of a yellow or fawn colour. It is evident however from its resisting the action of acids, that the colour of Nankin cotton cannot be due to the presence of a lake of alumina or any other base. In order to arrive at some conclusion regarding the nature of the colouring matter, it was necessary to employ large quantities of material, for though the colour looks intense when the cotton is viewed in mass, it is in reality produced by a small quantity of substance spread over a large extent of surface, in this respect resembling the colour of the petals of some flowers. I therefore had recourse to the plan adopted on a previous occasion, and described in a paper I had the honour of reading before this Society several years ago.* A quantity of yarn made entirely of Nankin cotton (from the coast of Coromandel) was submitted to the usual process of bleaching, and the dark brown liquid obtained by treating the yarn with boiling alkaline lye was mixed with an excess of acid which produced a dark brown flocculent precipitate. This was filtered off, washed with water, and then treated exactly in the manner described in the paper just referred to. It was found to contain the same substances as the precipitate obtained in the same way from alkaline lyes with which ordinary Indian or American cotton had been treated, viz., cotton wax, fusing at the same temperature and having the same general properties as that from ordinary cotton, a white crystalline fatty acid (probably margaric acid) pectic acid, parapectic acid, and lastly colouring matters. It is to the latter that the cotton owes its colour, for this colour is removed to a great extent by treatment with alkali, while the colouring matters are thrown down from the liquid

* Memoirs, 3rd Series, vol. IV., p. 95

on the addition of acid, and I therefore examined them with more care than the other constituents of the precipitate. These colouring matters I found to be at least two in number. One of them is easily soluble in alcohol and is obtained on evaporating the solution as a dark brown, shining, transparent resin. The other is almost insoluble in cold alcohol, but dissolves in boiling alcohol, and is deposited on the solution cooling in the form of a light brown powder. Their properties are in general the same as those of the analagous colouring matters from ordinary cotton. They contain, like these, C, H, N, and O, but in somewhat different relative proportions. Their composition in 100 parts I found to be as follows:

	A. Colouring matter soluble in cold alcohol.		B. Colouring matter insoluble in cold alcohol.
C	58·22	C	57·70
H	5·42	H	5·60
N	3·73	N	4·99
O	32·63	O	31·71

The composition of the analogous colouring matters from American cotton according to previous determinations was as follows:—

	A.		B.
C	58·42	C	58·36
H	5·85	H	5·71
N	5·26	N	7·60
O	30·47	O	28·33

The difference in composition, in the first case at least, is not greater than may be expected with substances of the purity of which, in consequence of their not occurring in a crystallized state, one can never be perfectly sure. On the whole, I think these experiments justify the conclusion at which I have arrived, viz., that the colour of Nankin cotton is due to the presence of bodies which are very similar to, if not identical with, those which cause the much fainter

tints of the ordinary kinds. They show too that the substances accompanying the cellulose (whether clothing the fibres, or contained in their interior) are the same with this variety of cotton as with all those previously examined.

"An Improved Method for preparing Marsh Gas," by C. SCHORLEMMER, F.R.S.

Everyone who ever had to prepare soda-lime knows that the preparation of this substance is a troublesome as well as a laborious process. Chemists will therefore hail with pleasure a paper "On the Determination of Nitrogen," by S. W. Johnson (*Liebig's Ann.*, 169, 69). He has found that in using the method of Varrentrapp and Will, soda-lime may be replaced by an intimate mixture, of about equal weights, of anhydrous sodium carbonate and dry slaked lime. It occurred to me that such a mixture might also be employed instead of soda-lime in the preparation of marsh-gas, and I found that by heating an intimate mixture of anhydrous sodium acetate with more than twice its weight of lime and sodium carbonate, a very regular and quiet evolution of marsh gas took place. The gas, thus obtained, always contains some acetone, which is easily removed by shaking it with water, or, better still, with a solution of acid sodium sulphite.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 13th, 1873.

Professor W. C. WILLIAMSON, President of the Section, in the Chair.

The PRESIDENT delivered the following address:

It being the wish of your Council that I should open our

new session by a few preliminary remarks upon our position in reference to the work which lies before us, I have not felt myself at liberty to decline acceding to their request.

I think we can scarcely meet in this hall without being in some degree stimulated to effort by the associations which cling to it. It is the hall rich in memories of White and of Perceval, of Dalton and of the two Henrys. It is the hall in which most of their discoveries were announced—discoveries which have given Manchester so distinguished a place in the scientific annals of Great Britain. It is to me a matter of no small pride and satisfaction that the present age will add its contribution to that illustrious roll of names, and that Joule and Fairbairn, Binney and Roscoe, will be remembered by our successors as men who in their several spheres of labour carried the torch of science with no uncertain grasp, and handed it on to their posterity shining with a light as brilliant as it gave forth when they received it from their distinguished predecessors.

Stimulated by these remembrances, we do well from time to time to take a conscientious view of our position in relation to the work which awaits us. We do not occupy our places in this room in the capacity of educators, but of investigators. Whatever duties of the former class may devolve upon some of us elsewhere, in this place we neither profess to be the teachers of the young, nor popularisers of science for the benefit of the adults who join in our assemblies. It is true that we may, in some humble measure, fulfil both these functions, but such fulfilments are but the collateral incidents of our position. Our proper duties are those of pioneers, endeavouring to carry light where all is as yet dark,—to dispel the thick mists of our own ignorance which still envelop us as with a pall, and which can only be dispelled by vigorous and combined efforts. It is our duty to try to discover unknown truths, to ascertain hitherto unobserved facts, and, if possible, to trace the relations which

subsist between them, as well as those which they bear to facts previously known. In this search for truth no new fact is so insignificant as to be unworthy of observation, since it may prove to be the falling apple capable of suggesting a law of the universe,—or the kite-drawn spark from the passing cloud which opens out a new world of light and force. If, then, in our researches we catch a faint glimpse of some such truth, let us pursue it diligently until we succeed in bringing it into clearer view. The ultimate use of which such truth is capable must not be the primary object of our pursuit. Many of our investigations must be carried on without reference to mercenary aims. At the same time whilst we thus follow truth for her own sake, let us not ignore the vital importance of applied science, or forget that those who so apply it are honoured instruments in alleviating the toils and increasing the joys of humanity.

How the existing band of active scientific workers is to be increased is one of the problems now occupying the minds of some of our ablest thinkers. The need for such an increase, especially in our own country, is admitted by all, but men differ as to the efficiency of the means suggested. Some, whose names stand high on the roll of the learned, think that the endowment of fitting men to be set apart as discoverers, will best meet the difficulty. I confess I have many fears as to the success of such a plan. Scientific research is rarely the child of wealthy leisure. It is true that you have, here and there, a Murchison or a Lyell, whom a fortunate combination of circumstances has freed from the necessity of labouring to win the means of existence, but who were yet more fortunate in possessing the vigorous energy which rendered powerless the paralyzing influences of wealth and station. As a rule, the wealthy classes have done but little scientific work, and such as have entered the field as vigorous labourers have rarely been those whose leisure was co-extensive with their

wealth. They were usually men, who, like the two Lubbocks, were actively engaged in the engrossing pursuits of the office or of the counting-house, but who still found time to labour in the scientific field and established their claim to stand abreast of the foremost in the army of workers. The fact is that too much leisure tends to enervate the mind, whilst the busy commercial or professional life stimulates the brain into ceaseless activity. This habitual vigour is restless for action, even in what with most men would be idle hours, and expends its surplus energy upon literary labours or scientific research. Such facts make me very dubious as to the advantages which would arise from the special endowment of men whose sole occupation in life should be scientific enquiry.

Except in some special departments of science accidental circumstances rather than a pre-arranged plan have much to do with determining the direction in which original investigations are carried on. Some small incident primarily directs an observer's attention to a special field of labour, and as he follows its leads the area expands before him and he ultimately finds himself deeply engaged in a career of important research. Of course I presume that early training has prepared him for the work. Now these facts appear to me to suggest the real remedy for the present scarcity of a high class of workers, and I think the times shew signs, however faint, of prospective improvement in this matter. We want a larger supply of sufficiently well endowed professorial chairs in which the obligatory duty of teaching shall stimulate a man to keep himself abreast of his age, but in which that duty shall not be so all-engrossing as to occupy the whole of his time. By such arrangements many needful things would be provided. We should have an enlarged body of men who would be competent to pursue original researches, because the previous training which their official position would involve would prepare them to recognise

what the Arctic navigator would term new leads, as well as give them the intelligent power to follow them out. I am satisfied that the two kinds of mental energies respectively involved in teaching and in pursuing new investigations act and re-act upon each other. That the man actively engaged in teaching is undergoing the best of preparations for other work, whilst the original investigator is of all men most likely to fulfil well the duties of a teacher, because he is more likely than other men to be animated by zeal and enthusiasm for the work upon which he is engaged. Original research, I believe, furnishes the best of training schools for the teacher, whilst the students will benefit by drinking at the running stream instead of at the stagnant pool. That a growing demand for teachers of physical science is springing up is obvious. The grammar schools of Manchester, Bradford, and Giggleswick may be taken as examples of the change which a more enlightened public opinion is producing in the respective various parts of the kingdom. When we remember at how recent a period a small band of workers like Huxley and Brodie first insisted upon the necessity for a great change in both the primary and the higher education of the youth of Great Britain I confess I marvel at the wondrous results that have already been attained, and I look forward into the future with sanguine hope. Meanwhile we experience a pressing want of a larger army of young workers. The case is one in which the old political maxim appears to be reversed in which supply will increase demand. It appears as if there were many great and venerable educational establishments in this country which would enter heartily into the work of science teaching if they could obtain competent and judicious teachers—men who would not merely cram their pupils' minds with isolated details but who were competent to make such teaching a real educational instrument and not merely an exercise of the memory. This want appears to me to indicate the more immediate

duty of government at the present time. The brief experiment tried at South Kensington, a short time ago, by Prof. Huxley, aided by Dr. Ray Lankester and Professor Michael Foster, is one which from its costliness must be prosecuted by the Government if carried on at all, because if it is to be productive of the full measure of the benefit to the nation of which it is capable, it must be carried on in a prolonged manner and on a very large scale. Such a work would, it appears to me, be accepted by the members of the House of Commons as involving a legitimate expenditure of the public finances, because it would be in strict accordance with that system of training of teachers to which they have already given their official sanction. Such a system, if regularly organised and perseveringly sustained, would tend to create an army of men competent to become original workers, and if this method was followed up by the establishment of local colleges in all the great centres of industry, there would be found permanent spheres of labour for the students thus trained. Two things needful to our national well-being would thus be attained; we should enrich the country by an extension of those scientific researches which within our own lifetime have proved to be so fertile a source of national wealth, and we should extend a scientific educational organisation which the fashionable quackeries of the day shew to be a necessary adjunct to that almost exclusively literary one which has so long monopolised the teaching functions in this country.

Mr. Charles Bailey exhibited specimens of *Carex punctata*, Gaudin, which he had collected in August last on the damp, narrow ledges of perpendicular rocks near a water-fall, on the north side of a small bay, named Waterwinch, about a mile north of Tenby, Pembrokeshire. This *Carex*, although it has long held a place in English floras, on the authority of some Cornish specimens seen by Dr.

Boott, has generally been admitted with doubt as an indigenous plant; many botanists believing that varieties of *Carex distans* had been mistaken for it. There are several localities in Ireland recorded as stations for Gaudin's plant; it has also been found recently in Scotland, and the Tenby locality above referred to re-establishes its right to be considered a British species.

November 10th, 1873.

CHARLES BAILEY, Esq., Vice-President of the Section, in the Chair.

"Remarks upon the British locality for *Lobelia urens*," by J. C. MELVILL, M.A., F.L.S.

The author being in the neighbourhood of Taunton this summer, determined to visit Axminster, the only known locality for this species in Britain, and set out for that purpose on the 1st of August.

The common where it was said to grow in 1836, has now been cultivated, and the old landmarks removed, and no trace of the plant was to be seen; however, a mile or two further on, beyond Shute Hill, he met with the lobelia in tolerable plenty, but exceedingly local. The flowers were in perfection, showing that the time of flowering mentioned in the various Botanical works is erroneous, where Autumn is stated to be the time. Mr. Melvill exhibited some dried specimens and distributed them among the members.

"On Lymexylon Navale," by JOSEPH SIDEBOTHAM, F.R.A.S.

Mr. Sidebotham referred to the various authors who have written about this species, the first and probably only British example of which was taken by Mr. Griesbach, in

Windsor forest, in the year 1829. It is said formerly to have been so abundant in the naval dockyards of Sweden, that Linnæus was consulted as to the best mode of stopping the injury done by it to the timber, an account of which he published in his *Iter Westrogoth.*

Last year several specimens of this insect were taken in Dunham park by Mr. Joseph Chappell, and Mr. Sidebotham, on visiting the tree, found the larvæ feeding in the wood.

In July of the present year the author visited the park again, and found various specimens, of both sexes, in the tree, and also on the wing, and expressed an opinion that the species would be found in other places now that its mode of life and habits had been investigated, of which he gave various particulars. He also exhibited specimens of the insects, and of the wood bored by them.

Mr. Sidebotham exhibited various forms of *Helix pisana,* from Tenby, and distributed specimens among the members.

Ordinary Meeting, December 2nd, 1873.

Rev. WILLIAM GASKELL, M.A., Vice-President, in the Chair.

Mr. Henry H. Howarth, F.S.A., was elected an Ordinary Member of the Society.

The following letter from HENRY BOWMAN, Esq., of Brockham Green, Surrey, dated 27th November, 1873, was read: —

Seeing the report of Prof. Reynolds' experiments on the nature of the explosive force of lightning, it occurred to me that the Society might be interested in an account of a remarkable thunderstorm which took place here on Friday the 7th inst.

I was not at home at the time, and did not hear of the storm until my return the following week.

The Vicar of this place sent a short notice of the storm to the "Times," copy of which is inclosed, together with some further particulars which he has kindly jotted down.

(COPY.)

Times of Nov. 12, 1873:

"The Rev. Alan Cheales ...... writes to us ...... a remarkable storm burst over the valley between Dorking and Reigate on Nov. 7....... In general we have a remarkable immunity from thunderstorms, which draw away along the Downs to the north of us. The storm came up suddenly from the N.W. ...... I have just returned from inspecting an oak tree in Betchworth Park ...... which was completely cleft in two ... .. the tree has been chopped down within about 10 feet of the ground ...... the girth is about 8 feet."

My dear Sir, Nov. 20, 1873.

In reply to your enquiries relative to the very remarkable thunderstorm of Nov. 7, mentioned in the *Times* of Nov. 12, I

have much pleasure in putting before you such other particulars as I have been able to gather.

There was no warning whatever, a black cloud rolled up, a little rain, and then two tremendous flashes.

The tree struck was a fine young oak, half covered with ivy, in a row with other oaks, some also having ivy on them.

E. Batchelar, painter at Dorking, 2 miles off, describes a tremendous flash of blue light, and thunder instantly.

Rev. G. R. Kensit, walking in fields about 1 mile off, describes a ball of blue fire which seemed to fall on his foot.

Martha Rapley, coming home through Betchworth Park with perambulator and two children, was only 300 or 400 yards from the tree; was so stunned and blinded that she saw nothing — perambulator seemed wrapped in flames — she did not know whether the children were dead or not until she got home. Neither injured.

J. C. Richards, Esq., of Boxhill Farm, had a sheep killed about ½ a mile off; he seemed to think it was killed by the same flash that split the tree. There was only one other flash.

The tree, unfortunately, was removed at once by Mr. Hope's woodreeve. I could not see that any part was blackened. Mr. Richards observed to me that no conceivable human force could have so split it.

Part of the fibre had been made to writhe round in a most remarkable manner.

Huge splinters (3 feet by 3 or 4 inches) were chopped out and lay a few feet off.

The tree was divided and hung on each side of the fence about 8 or 10 feet from the ground. The lightning seemed to have run down thence along the outside bark into the ground; but the marks were slight. The tree was still solid below the cleft.

I am yours faithfully,

Alan Cheales.

Ordinary Meeting December 16th, 1873.

E. W. Binney, F.R.S., F.G.S., Vice-President, in the Chair.

Mr. James Heelis was elected an Ordinary Member of the Society.

The Chairman said that since the last meeting the Society had lost one of its most illustrious members by the death of Professor Louis Agassiz, the great naturalist, who had been an Honorary Member for above thirty years. He (the Chairman) had the honour of being personally acquainted with the deceased, having been brought into communication with him during the publication of his great work, "Recherches sur les Poissons fossiles," and having had the pleasure of supplying him with specimens for its illustration. In the Royal Society's Catalogue is a list of 130 scientific memoirs he gave to the world. The reputation of Agassiz as one of the foremost men of his day in natural history is too well known to need any tribute from me, but after a lapse of thirty years his amiable manners and his great kindness in cheerfully imparting his vast stores of knowlege to the humblest student are fresh in my memory. He was one of the kindest and heartiest of men, and his fine and manly countenance at that time was the picture of health and good nature, and truly reflected the genial soul within. In great and small matters he was equally punctual and correct. All who ever allowed him to make use of their specimens must well remember the ample acknowledgments he made and the scrupulous care with which they were returned; and some of the first living palæontologists might learn a useful lesson in this respect from the illustrious dead. With Agassiz it may be truly said that it was hard to decide whether his head or his

heart most deserved our admiration. The world has to lament the death of a great and good man, and this Society one of its greatest ornaments.

"Method of Construction of a New Barometer," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., President.

The condition of the instrument placed on the 18th of March in the Society's Hall proves that it is possible to use sulphuric acid on the top of the mercurial column without chemical action taking place. I have therefore proceeded to prepare other tubes with a view to test, by practical work, the merits of the new contrivance.

A tube of about $\frac{3}{16}$ inch bore is selected. It is first cleaned by drawing a knotted string through it. It is then bent to the siphon shape; and near the longer end it is drawn to a capillary tube. It is then washed with nitric acid; afterwards with sulphuric acid. The sulphuric acid is then drained off. Mercury is then poured into the short limb. The end of the longer limb is then attached to my mercurial exhauster. On working this the mercury rises in the tube, and, being replenished by pouring it into the short limb, soon arrives at the height due to the atmospheric pressure. It carries with it the acid left adhering to the sides, so that after a few hours half, or, what is better, one third of an inch of acid stands above the mercury. Small bubbles of air are seen to arise; but by leaving the tube in connexion with the exhauster for a day or two these finally cease. Mercury is then poured into the short limb until that in the longer rises nearly to the capillary part of the tube. This is then sealed and detached from the exhauster. Mercury is then removed from the shorter limb until it stands in the long one at a convenient height. Sulphuric acid is then introduced into the short limb until it forms a column equal to that in the longer limb. A small tube is finally attached to the short limb, and dipping a

little way into a small bottle containing a small quantity of sulphuric acid, prevents the access of moist air into the short limb.

The tube thus completed possesses the following advantages: — 1st. There is the utmost facility in the movement of the column, so that the most minute changes of pressure are at once registered without any dragging. 2nd. The depression produced by capillary action is reduced to one half, so that the siphon arrangement can be satisfactorily used as affording an accurate neutralization of capillary action.

Mr. BROTHERS exhibited the plates forming the first part of the Holborn Society's photolith reproduction of Hans Burgman's *Triumphs of Maximilian I.* The designs are engraved on wood and printed on separate sheets, but the set shown were mounted so as to exhibit the artist's intention — that of a triumphal procession. This remarkable work is considered to be one of the finest specimens of wood engraving.

Mr. BAXENDELL read the following letter from Professor C. PIAZZI SMYTH, F.R.S., Astronomer Royal of Scotland: —

Referring (as the prompt and frequent publications of your Society so easily and agreeably enable one to do) to Professor Osborne Reynolds's triumphant proof, on November 18th, that his glass tubes were strong enough to act as guns,—and also that 1·5 inches depth of powder produced nothing like the force exerted by the electrical discharge; and that that electrical discharge acted by means of conversion of water suddenly into steam, as when lightning rends a tree;— may I beg to offer two remarks?

(1) The soundness of the discussions before the Society on the recent wood-struck case near Manchester, is evident by a similar conclusion arrived at by the British Associa-

tion when at Edinburgh in 1850. For a tree in the neighbourhood having been struck and specially shattered into thin plates of wood, during the week of congress, was formally examined by a deputation of the Association, and the lightning was held to have exploded the watery matter of the sap-vessels.

(2) That water is a far more powerful exploder than gunpowder if you can get it (the water) to explode at all, is now experimentally proved by Professor Osborne Reynolds's electrical experiments; and did occupy my attention many years ago, on comparing the far larger increase of space occupied by exploded water in the shape of steam, than by exploded gunpowder in the shape of its permanent gases.

The difficulty however is, to get the water to explode, and not to pass off merely into steam; a difficulty well illustrated by any and every accession of dampness to gunpowder fired in the usual way, decreasing, instead of increasing, the gunpowder's explosive force.

In order to try to explode water, at that time, I melted a large ladle full of lead; put upon the fluid and almost red-hot surface a drop of water and tried various devices to bring it under the influence of the heat; but even when forcibly attempted to be pushed under the melted lead, the water ran with vehemence up the substance of the wooden probe employed, and refused to have anything to do with the fluid lead, which consequently remained undisturbed.

But when I next took a smaller iron ladle, put a drop of water on the bottom of it, and gave therewith a little pat to the surface of the melted lead, instantly the whole contents of the great ladle were scattered to the winds, and only a few grains were recovered. Explosion of water had apparently taken place with excellent effect.

Then came a question as to repeating such an explosion

at small intervals of time, in a safe manner, so as to have an explosion engine; in which, if all the heat of the coal could be used in exploding water, rather than in raising steam, a surprising economy of fuel should result.

But as no progress was made in such an engine, I can only refer to some old accounts of an explosion in a copper foundry, where the great establishment was literally blown up, it was said, by a workman simply spitting into a vessel of melted copper. The mere amount of steam raised from the saliva would evidently have been of no practicable avail for either good or evil, even if employed in the best modern expansive engine on the thermo-dynamic principles; but, as an explosive, its energy would seem to have been so vast, that I must hope for further development of the subject at the hands of the able men of science in the Manchester Literary and Philosophical Society.

"On the Destruction of Sound by Fog and the Inertness of a Heterogeneous Fluid," by Professor OSBORNE REYNOLDS, M.A.

1. That sound does not readily penetrate a fog is a matter of common observation. The bells and horns of ships are not heard so far during a fog as when the air is clear. In a London fog the noise of the wheels is much diminished, so that they seem to be at a distance when they are really close by. On one occasion during the launch of the Great Eastern the fog was reported so dense that the workmen could neither see nor *hear*.

2. It has also been observed that mist in air or steam renders them very dull as regards motion. This is observed particularly in the pipes and passages in a steam engine. Mr. D. K. Clark found in his experiments that it required

from 3 to 5 times as much back pressure to expel misty steam from a cylinder as when the steam was dry.

3. My object in this paper is to give and to investigate what appears to me to be an explanation of these phenomena; from which it appears that they are intimately connected, that, in fact, they are both due to the same cause. This explanation will be the clearer for a few preliminary remarks.

4. The nature of a fog and the manner in which the small spherical drops are suspended against their weight is well understood. So long as the fog is at rest or moving uniformly, the drops being heavier than the air tend to sink like a stone in water, and consequently they are not at rest in the air but are moving through it with greater or less velocities according as they are large like rain or small like haze. This motion is caused entirely by the difference in the specific gravity of the air and water, if the drops were merely little hard portions of air they would have no tendency to descend.

In some fogs the drops are so fine that they appear to be absolutely at rest, and will remain for a long time without any appreciable motion. The force which retards the downward motion of the drops is the friction of the air, and this is proportional to the surface of the drop and the square of the velocity. As the drops get smaller their weight diminishes faster than their surface, and consequently the friction will balance the weight with a less velocity. The exact law is that the velocity caused by the weight of a drop is proportional to the square root of its diameter.

This is the general explanation of what goes on under the action of gravity when the fog is at rest or moving uniformly, and we may make use of it to illustrate what goes on when the fog is subjected to accelerating or retarding forces.

5. If we imagine a vessel, full of such a compound as the fog is made of, to be set in motion or stopped, the accelerating or retarding force will have to be transmitted from the sides of the vessel to the fluid within it by means of pressure. These pressures will act equally throughout the fluid, and if the fluid were homogeneous they would produce the same effect throughout it, and it would all move together but the pressures will obviously produce less effect on the drops of water than they do on the corresponding volumes of air, and the result will be that the drops of water will move with a different velocity to the air—that the drops of water will in fact move through the air just as they do under the action of gravity. In fact, if the air is subject to an acceleration of 32 feet per second the effect on the drops (their motion through the air) will be the same as that due to their weight. It is easy to conceive the action between the air and the drops of water. If a mass of air and water is retarded it is obvious that the water, by virtue of its greater density, will move on through the air. This property has, in fact, been made use of to dry the steam used in steam engines. The steam is made to take a sharp turn, when the water, moving straight on through it, is deposited on the side of the vessel.

6. Owing to this motion of the water through the air it would clearly take longer with the same force to impress the same momentum on foggy air than on the same when dry. This is obvious, for at the end of a certain time the particles of water would not be moving as fast as the air, and consequently the air and water would have less momentum than the same weight of dry air all moving together: that is to say, if we had two light vessels containing the same weight of fluid, the one full of dry air and the other full of fog, and both subjected to the same force for the same

time, at the end of this time, although they would have exactly the same motion, their contents would not, for the drops of water in the fog would not be moving so fast as the vessel. Now the energy expended on each of these vessels would be the same, but, inasmuch as the effects are different, the energy acquired by the foggy air would be less than that acquired by the dry air, the difference having gone to move the water through the air: that is to say, it would require more pressure to impress in the same time the same velocity on foggy air than on dry air of the same density.

7. This then fully explains the dulness with which foggy air acquires motion. In the passages of a steam engine the steam is subjected to continual accelerations and retardations, each of which requires more force in the manner described with misty than with dry steam, and at each of which the particles of water moving through the steam destroy energy in creating eddies.

8. Although not so obvious, the same is true in the case of sound. The effect of waves of sound traversing a portion of air is first to accelerate and then to retard it. And if there are any drops of water in the air these will not take up the motion of the air so readily as the air itself. They will allow the air to move backwards and forwards past them, and so cause friction and diminish the effect of the wave as it proceeds, just as a loose cargo will diminish the rolling of a ship.

9. It is important to notice that this action of the particles of water is not analogous to their action in reflecting the waves of light.

It has been assumed as an explanation of the action of fog on sound that the particles of water break up the wave of sound by small reflections in the same way as they scatter the waves of light. The analogy however is not

admissible; for in the case of light the wave length is shorter than the thickness of the drops, and the surface of the drop acts in the same way as if the drop were of large extent; but in the case of sound the wave's length may be thousands of times the thickness of the drop, and instead of the whole wave being reflected it will only be a very small portion of it. Even this portion can hardly be called a reflection; it is due to the motion of the air past the drops like the waves of sound caused by a bullet, or the waves thrown off by the bow of a ship.

10. A certain portion of the resistance which the air offers to the motion of the water through it is this—what is called in naval science *wave resistance;* but it can be shown that the proportion of this resistance to the resistance in causing eddies diminishes with the velocity, and consequently it can have very little to do with the effect of the drops of water on the waves of sound, in which the velocity of the water through the air must be very small.*

11. So far, then, I have shown the manner in which the fog diminishes the sound; it remains to consider the connection between the size of the drops and their effects. I am not aware that any observations have been made with respect to this. I do not know whether it has ever been noticed whether a fine or a coarse mist produces the most effect on sound. It does not appear, however, that rain produces the same effect as fog; and considering rain as a coarse fog, we must come to the conclusion that a certain degree of fineness is necessary.

If we examine theoretically into the relation between the size of the drops and the effect they produce, always assuming

* This reflection has nothing to do with the reverberation from clouds which occurs in a thunderstorm, which is probably due to the different density of the clouds, and takes place at their surfaces.

the same quantity of water in the air, we find in the first place that if the air is subjected to a uniform acceleration, which acts for a sufficient time for the drops to acquire their maximum velocity through the air, the effect of the drops in a given time—that is to say, the energy dissipated in a given time—is proportional to the square root of the diameters of the drops. This appears from the action of gravity. As previously stated, the maximum downward motion of the drops; and hence the distance they will have fallen in a given time and the energy destroyed is proportional to the square root of their diameters. Hence where the acceleration acts continuously for some time, as would be the case in a steam pipe, the effect will increase with the size of the drops.

This effect may be represented by a parabolic curve in which distances measured from the vertex along the axis represent the size of the drops and the corresponding ordinates represent their effect in destroying energy.

If on the other hand the acceleration alternates very rapidly then there will not be time for the drop to acquire its maximum velocity, and if the time be very short the drop will practically stand still, in which case the effect of the drops will be proportional to the aggregate surface which they expose. And this will increase as the diameter diminishes, always supposing the same quantity of water to be present.

This latter is somewhat the condition when a fog is traversed by waves of sound, so long as the drops are above a certain size; when, however, they are very small, compared with the length of the waves, there will be time for them to acquire their maximum velocity. So that starting from drops the size of rain, their effect will increase as their size diminishes, at first in the direct proportion, then more and more slowly until a certain minuteness is reached, after

which, as the drops become still smaller, their effect will begin to diminish, at first slowly, but in an increasing ratio, tending towards that of the square root of the diameter of the drops.

This effect may be represented by a curve which coincides with the previously described parabola at the vertex, but which turns off towards the axis, which it finally approaches as a straight line.

This completes the investigation, so far as I have been able to carry it. The complete mathematical solution of the equations of motion does not appear to be possible, as they are of a form that has not as yet been integrated. However, so far it appears to me to afford a complete explanation of the two phenomena, and further to show, a fact not hitherto noticed, that for any note of waves of sound there is a certain size of drop with which a fog will produce the greatest effect.

"The Chemical Constitution of Bleaching Powder," by C. SCHORLEMMER. F.R.S.

In his classical research "On the Compounds of Chlorine with Bases,"* Gay Lussac has shown that the bleaching compounds formed by this reaction are not direct combinations of chlorine and a base, as Berthollet believed, but that a hypochlorite and a chloride are produced simultaneously, according to the equation

$$2KOH + Cl_2 = KOCl + KCl + H_2O.$$

When to the compounds thus formed a small quantity of a mineral acid is added, hypochlorous acid is set free, whilst by adding the acid in excess chlorine is obtained; because in the latter case the hydrochloric acid acts on the hypochlorous acid in the following way:

$$ClH + ClOH = Cl_2 + H_2O.$$

* "Comptes Rendus," XIV. 927.

As a ready method for preparing a dilute solution of hypochlorous acid, Gay Lussac recommends to distil a solution of bleaching powder with a quantity of dilute nitric acid which is just sufficient to liberate the hypochlorous acid.

According to Gay Lussac's view, bleaching powder is a mixture of calcium chloride and calcium hypochlorite, and the same view is held by most chemists. Professor Odling has however pointed out that, calcium being a dyad metal, the constitution of bleaching powder was probably $Ca \left\{ \begin{array}{l} Cl \\ OCl \end{array} \right.$ or it was at the same time a hypochlorite and a chloride. Of course both views explain equally well the formation of hypochlorous acid by Gay Lussac's method. I read therefore with great surprise a paper by Goepner (Dingler's *Polytechn. Journ*, 209, 204), in which he states that bleaching powder is nothing but a simple combination of lime and chlorine, which by acids is again resolved into its constituents without the least trace of hypochlorous acid being formed. He says that, although the preparation of hypochlorous acid by this method is described in all hand books as if this experiment had been made hundreds of times, this is a mistake, and the reason why this error has maintained itself so long in chemical literature is that hitherto no reaction was known by which free chlorine and hypochlorous acid could be readily distinguished. But such a reaction has now been found by Wolters, who has shown that when chlorine-water is shaken with an excess of mercury only mercurous chloride is formed, while with aqueous hypochlorous acid it yields a brown crystalline oxychloride of mercury, which is readily soluble in hydrochloric acid, and thus offers a ready means of the qualitative as well as quantitative determination of hypochlorous acid in the presence of free chlorine.

In employing this reaction for detecting hypochlorous acid in the liquid which was obtained by distilling bleaching powder with a small quantity of hydrochloric or sulphuric acid, Goepner could not find a trace of hypochlorous acid, but only free chlorine.

I have already mentioned that he says the preparation of hypochlorous acid by this method is described in the books as if this experiment had been repeated hundreds of times. Now this experiment has been repeated many hundred times in our laboratory only. Professor Roscoe shows it every year in his lectures, and all our students in the course of their practical work perform it, and find that the perfectly colourless distillate is a much more powerfully bleaching agent than freshly prepared chlorine-water. This is quite sufficient to show that the liquid contains hypochlorous acid. But why did Goepner fail in detecting it? Perhaps it was the fault of the analytical method? To decide these questions I prepared hypochlorous acid by distilling solutions of bleaching powder with dilute nitric and sulphuric acid and shook the colourless distillates with mercury. In every case the brown oxychloride was formed in quantity and possessed all the properties which Wolters has assigned to it, while by shaking chlorine-water with mercury only calomel was formed. From a careful perusal of Goepner's paper I was unable to find the cause of his failure.

Another argument against the existence of a hypochlorite in bleaching powder is, according to Goepner, the following. The chlorine which is used in the manufacture of bleaching powder always contains free hydrochloric acid, and thus in bleaching powder more calcium chloride will always exist than would correspond with Gay Lussac's formula. Now when bleaching powder is exhausted successively with

small quantities of water, the excess of calcium chloride is always found in the first solutions, whilst those following contain calcium and chlorine in the proportions corresponding to the empirical formula $CaOCl_2$. This fact, however, only proves that bleaching powder is not a mixture of calcium chloride and hypochlorite, but that the bleaching compound contained in it has the constitution which Professor Odling has assigned to it.

Professor Williamson has shown that an aqueous solution of hypochlorous acid may also be obtained by suspending finely divided calcium carbonate in water and passing chlorine into the liquid until the carbonate is dissolved and then distilling the solution. In this reaction the compound Ca(OCl)Cl is probably also first formed and acted on by an excess of chlorine in the following way:—

$$Ca\left\{\begin{matrix}Cl\\OCl\end{matrix}\right. + Cl_2 = Ca\left\{\begin{matrix}Cl\\Cl\end{matrix}\right. + Cl_2O.$$

Ordinary Meeting, December 30th, 1873.

Rev. Wm. Gaskell, M.A., Vice-President, in the Chair.

Mr. E. W. Binney, F.R.S., F.G.S., said that in the Liverpool papers a good deal of discussion had lately taken place on the rapid growth of the bar at the entrance into the Queen's Channel. This is a subject not only of local but of national interest. Having had occasion to pass over the bar frequently in his passage to the Isle of Man, he had his attention directed to the matter, and his friend our worthy President had supplied him with a copy of an interesting article from the *Liverpool Albion* of the 18th November last. Adam Evans appears to attribute this evil to the emptying of mud into the Mersey, and the writer in the newspaper states as follows: "Nothing is so important to the prosperity of the port of Liverpool as the maintenance of its entrance channels at sufficient depth. Several times lately the question has been named at the Dock Board, and more particularly at the meeting of the Board last week. It appears that Admiral Evans, acting Conservator of the River Mersey, has been for a long time urging the construction of steam barges to take the deposits from the docks (which deposits consist not of soluble kinds only, but also of coal, coal dust, and all other matter which happens to fall overboard from the ships in the docks), and to convey them outside the port. That this is no new view of Admiral Evans's is shown by the fact that in his report for 1843, as to the impropriety of discharging mud and other matter from the flats into the river, he pointed out the dangers attending the practice; and if at that time he considered the system bad, how much more so is it now? The deposits

then were about 200,000 tons annually; now—the acreage of the docks having increased from about 100 to 500 or 600 acres—they are probably at least 600,000 tons annually, and all this refuse is discharged into the river. To elucidate the matter we may give the following extracts from Admiral Evans's report of 1843 relative to the causes which to the extension of the Victoria Bar, and of the effects of depositing mud and other substances in the bed of the river: "The extension of Victoria Bar is occasioned by a greater quantity of detritus being held in suspension by the ebb than by the flood tide, which causes the point of deposit to continually extending outwards to where the stream of ebb is exhausted. Although this to a certain extent is of common occurrence at the mouth of all estuaries, yet the excess of matter held in solution by the ebb over the flood tide in the Mersey is in a great measure to be attributed to the habit of throwing into the river the whole of the mud and sand 'daily' extracted from the Liverpool docks, amounting from January, 1843, to January, 1844, to 213,000 tons! This enormous quantity is taken out of the docks by steam dredges, discharged into mud flats, which are towed out by steamers every tide, day and night, except on Sundays, and emptied into the middle of the river at the worst possible time of tide—namely just before high water, so that part of the mud is taken up the river to feed the Pluckington and other banks; part is precipitated to the bottom, taking with it the sand held in suspension by a full tide; and the remainder is filtered through the different seaward channels, leaving a portion of its silt in them.

This practice, so mischievous to the navigation of the Mersey, and detrimental to the best interests of Liverpool, is continued under the sanction of a local Act of Parliament (6th of George IV., cap. 117), which was passed in the year 1825. Since the passing of that Act the dock space of Liverpool has not only been doubled, and annually increasing, but

the ships are of a much larger class, requiring a greater depth of water both in the docks and channels. If, therefore, the Act of Parliament above alluded to applies to the docks made and deepened since its passing in 1825, to those docks now in progress, and to all that may be hereafter be constructed, the amount of clay and mud thrown into the river will be enormous, endangering the navigation at the very time, and in proportion as the increase both in size and number of the ships requires the channel to be kept as deep and clear of deposit as possible.' In consequence of this report the practice of discharging mud boats on the flood tide was abandoned, and they have since been discharged during the first quarter of ebb. That the views then adopted by Admiral Evans were right is shown by the present condition of the main or Queen entrance channels into Liverpool, which are now only about seven to eight feet on the bar at low water spring tides, instead of about twelve feet, so that small tug boats cannot now get in and out where comparatively large steamers used to be able to pass. An illustration of this fact was afforded the other day, when one of the Isle of Man boats had to wait some time before she could cross the bar. With regard to Rock Channel, the depth of water on the bar is now only 18 inches at low water spring tides. It may be thought that in going back to Admiral Evans's report for 1843 we have travelled out of our way to manufacture arguments against the present system of depositing mud in the Mersey; but if we return to the Admiral's report for 1872, we find the views of 1843 confirmed."

No doubt the throwing in of about 2000 tons of mud into the Mersey near Liverpool every working day is a thing which ought not to be allowed by the conservator of that river, but it is highly probable that this is only one of the causes of the decreased depth of water over the bar of the Queen's channel. It is now pretty well known that encroachments and embankments on estuaries and river courses

cannot be made without some detriment to such places. The natural flow of water is generally the best for cleansing and sweeping out a river or estuary, and such agency may be helped and diverted, but it can never be opposed or obstructed with impunity, Owing to the want of reliable experiments it is not yet well known at what speed a current of water ceases to cut a bed of clay and begins to lift up the course of such water. All we know broadly is that mountain streams deepen and low country streams raise their beds.

The estuary of the Mersey and its deep channels have been kept open chiefly by the ebb tide and the back waters of the Mersey, Weaver, and other streams. Of late years the shores on both sides of the Mersey have been extended into the water way by the construction of new docks and other works so that the channel is now considerably less in width than it formerly was, and consequently less water enters the river from the sea and flows up it than used to do, and thus diminishes the power of the ebb tide. True it is that the contraction of the river has made the current of the ebb for some time stronger opposite to Liverpool, and thereby increased the scour and cutting power there, but the materials removed soon fall to the bottom when the water course becomes wider and the current less, so this increased scour will not compensate for the loss of water caused by the obstructions to the flood tide but actually increase, to a considerable extent, the deposits of the removed materials in the vicinity of the bar. It is quite clear that the emptying of dock mud into the river is an evil, but what is it when compared to the solid matter brought down daily and in freshes by the inland streams feeding the Mersey.

My object in making these remarks is to direct attention to the subject of the lowering of the depth of water over the Queen's Bar, which is of the utmost importance to steamers leaving and arriving at the tidal harbours about

high water and reaching the bar at near low water. This is especially the case with the Isle of Man steamers during the greater part of the year. No doubt that particular winds may at one time increase and at another diminish the sand on the bar, but it appears pretty certain that for some considerable number of years the water has gradually become shallower at the entrance of the Queen's Channel. The true cause of this evil has first to be ascertained and then no doubt the authorities at Liverpool will lose no time in remedying it.

Mr. Binney also exhibited to the meeting a very perfect specimen of a fossil fish, measuring from head to tail 14 inches. It was discovered in the cannel seam of the Pirnie Coal Company at Methill in the county of Fife. He considered it to be a small *Magalichthys Hibberti.* Although fragments of this fish are very frequently met with in the coal measures, it is not often that a specimen is found in so good a condition.

Ordinary Meeting, January 13th, 1874.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

Mr. Rooke Pennington, of Bolton, was elected an ordinary Member of the Society.

A drawing was shown representing some further improvements of Dr. Joule's mercurial air exhauster described in the Proceedings of February 18, 1873.

In the section represented by Fig 1, W W is a wooden frame; P a pulley for raising or lowering a flask of mercury held in a wooden box, M, working in a slide; ṡsss are india rubber stoppers; E is the exhauster; *t*, *e* the entrance and exit tubes; *g* the gauge; *f* a funnel to admit sulphuric acid; B, B moveable brackets to support any apparatus.

In Fig 2 the exhauster is drawn to a larger scale. *t*, *e* are the entrance and exit tubes, fitting tightly in an india rubber disc *a*, which disc is kept tightly pressed against the exhauster by means of the ring, *b*, *b*. The mercury is represented sunk below the entrance tube, as is the case when the moveable flask is in its lower position. On raising the flask by means of the pulley, the mercury rises in the exhauster and forces any air it may contain into the upper part of the exhauster by raising the india rubber plug. The air then makes its exit through the pipe *e*. This latter is also used for withdrawing the acid which gradually accumulates.

Fig 3, also drawn to a large scale, represents a convenient means of introducing sulphuric acid for removing aqueous vapour, or to let air into the apparatus. The orifice at the bottom of the funnel is about $\frac{1}{100}$ of an inch diameter to prevent violent action.

It may be useful to mention that the junctions are made with black india rubber tube fastened by softened iron wire.

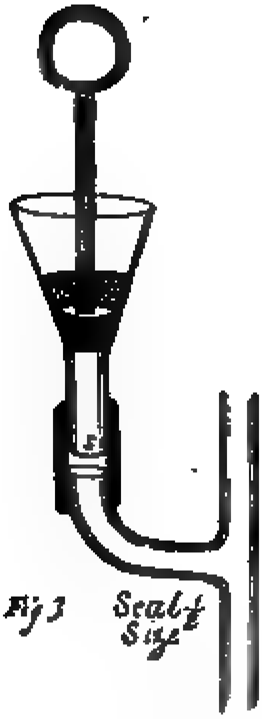
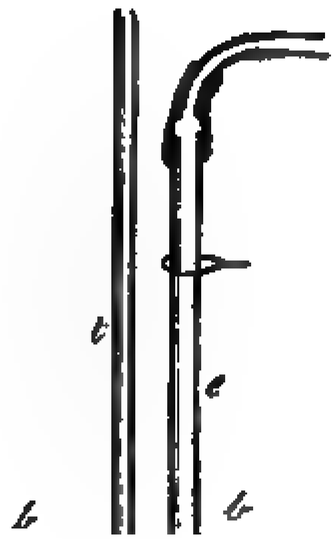


Fig 3 Scale ¼ Size



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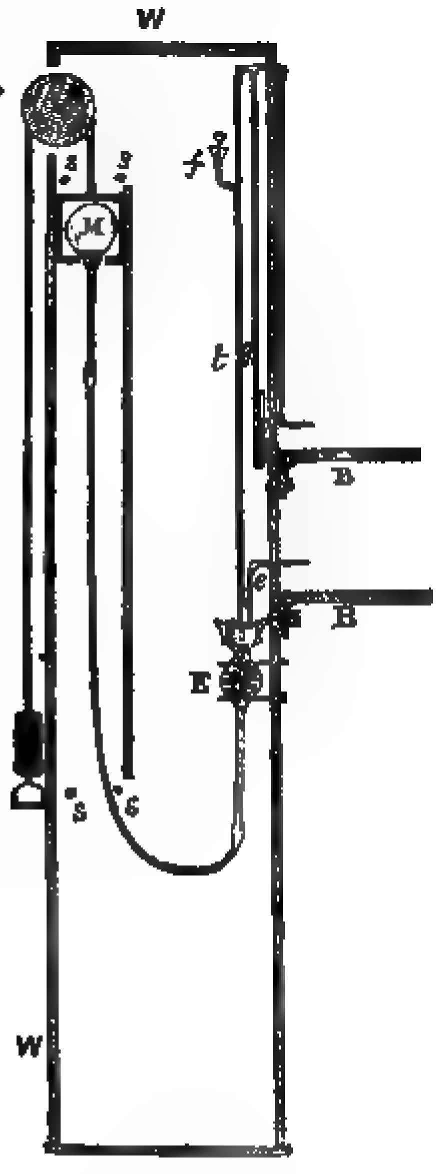

Fig1. Scale $\frac{1}{12}$

"On the Influence of Acids on Iron and Steel," by WILLIAM H. JOHNSON, B.Sc.

In a paper published in the Proceedings of the Society for March 4th, 1873, I mentioned that if a piece of steel wire be immersed in hydrochloric or sulphuric acid for ten minutes or more, and then well washed with water and dried, that on breaking it bubbles were not seen to rise through the moisture on the surface of the fracture as was the case with *iron* wire. Subsequent experiments made under the microscope with a power of 250 diameters; however, show that very small bubbles are given off with great rapidity, sometimes from the whole, sometimes from part only of the fractured surface.

This difference in the behaviour of iron and steel is most likely connected with the difference of molecular structure. Thus the fracture of a steel wire containing say ·75 per cent of carbon, when seen under the microscope presents a tolerably flat surface, composed of innumerable small, sharp crystalline points, while that of iron is rough, more or less fibrous or mossy, and the fibres do not end in sharp points. These fine crystalline points in the steel, as is well known, must facilitate the evolution of bubbles; consequently they are very small, rapidly given off, and hence invisible to the naked eye, whilst the absence of these points in iron causes the small bubbles to collect into larger ones, which are readily seen.

The less carbon a steel contains the more its fracture will resemble iron, so in a steel containing only ·21 per cent. of carbon, small bubbles may sometimes be seen by the naked eye.

About 5 oz. of iron wire ·125 in. diameter, after 10 days' immersion in hydrochloric acid 1·20 s.g., was well washed in water, dried and placed in a glass tube heated to a temperature of a little over 100° C. by a sand bath. Each end of the tube was connected with a bottle containing nitrate of

silver solution. A current of air was then slowly drawn through the tube for two to three hours without forming any precipitate however of chloride of silver; but the surface of the iron was covered with a coating of oxide, or in all probability, oxichloride of iron.

Thick pieces of iron ·450 in. diam. were found to redden blue litmus paper slightly when applied to the fracture after the iron had been immersed 12 hours in hydrochloric or sulphuric acid.

Mr. J. CARTER BELL communicated a series of meteorological observations which had been made daily during the months of May, June, July, and August, 1873, at Tumaco, New Granada, S. America, by Mr. G. WILCYNSKI.

"On Crystalline Sublimed Cupric Chloride," by S. CARSON, (Student in the Chemical Laboratory of the Owens College).

Cupric Chloride prepared by burning copper in dry chlorine gas or by heating the anhydrous salt to 200° is obtained either as a brown sublimate or as a brownish yellow powder.

A mass of a copper compound crystallized in needles, many exceeding 5mm. in length, as found in the decomposer of the Deacon-Chlorine process was forwarded to Professor Roscoe, by Mr. Worsley, of the Netham Chemical Works. The crystalline mass was collected from the space above the marbles, impregnated with copper sulphate at the top of the compartments. The temperature of this space is always necessarily a little lower than that of the spaces between the marbles where the action takes place, and to this is attributed the deposition of the compound.

I have made several analyses of these crystals, the results of which show that they consist of anhydrous cupric chloride, mixed with a small quantity (about 2 per cent) of an insoluble oxychloride. The following is a mean of several analyses of the soluble portion:—

	Calculated.	Found.
Copper	47·172	46·909
Chlorine	52·828	53·091
	100·000	100·000

The formation of this crystalline sublimate appears to take place as soon as the temperature of the marbles covered with sulphate of copper reaches about 800°. In all probability a formation of copper chloride is constantly occurring as a necessary step in the decomposition of the hydrochloric acid and air, and when the temperature reaches the volatizing point of the chloride these crystals appear.

As the greatest amount of decomposition of the hydrochloric acid takes place close upon the temperature at which the chloride sublimes, the formation of this salt and the consequent loss of copper has been a fertile source of annoyance to the manufacturer.

Mr. Deacon has recently completely overcome this difficulty by the addition of sodium sulphate to the copper sulphate with which the marbles are impregnated.

The presence of this salt prevents any formation of copper chloride, sodium chloride being volatilized, and copper sulphate remaining behind. This reaction is well seen by the change of colour from green to blue produced when a solution of sodium sulphate is added to one of cupric chloride.

"Memorandum on Brown-stapled Cotton," by Major R. Trevor Clarke. Communicated by Dr. E. Schunck, F.R.S.

The nankeen colour in cotton staple may be considered, I think, as a normal variational state, rarely met with in the present day, but very probably natural to the wild forms now nearly extinct and very imperfectly known. The two plants which have the best claim to be considered wild species that I have met with, namely, the Polynesian plant of Nuttall, and the Santo Paulo one of Mr. Aubertin,

have both of them yellow cotton. The coloured state however is common to several cottons, probably to all. We find it in the hill cotton of Assam and in samples from China (Ning-Po), both *Goss. herbaceum* proper, and I am told it is not unfrequently found in the fields of this species in India and elsewhere.

In India however the sort actually in cultivation is the yellow form of *G. hirsutvm* (Orleans), and my samples from Malta are of the same kind. The only use that I know of for this staple is to make the cotton blanket clothing of the Afghans, and I think also the Kabyles use it for the same purpose.

It seems to be an object of regard, and even veneration, amongst the aborigines of various countries. In Peru the country people weave a striped cloth, white and yellow, from it, and the bodies of their ancient princes the Incas were found to have been buried enveloped in the rich brown wool of a coloured form of the large native plant.

The paler brown staple alluded to as coming from Africa is the produce of a coarse kind of Egyptian, and is of stronger quality than most. The curious kidney cotton also assumes this appearance, as I have samples of it from Parahyba del Norte, and have raised plants from its seeds. In all cases the staple suffers an unfavourable change when assuming this state, and is invariably more or less weak and short.

The occurence of the yellow state in so many different kinds will go far to account for the diversity in quality alluded to.

A tendency to brown coloration seems peculiar to the genus; it shows itself in the dark hue of the expressed oil, and when injury to the seed and capsule, by insects or otherwise, has occurred, the extravasation has resulted in stains upon the, properly, white fibre.

In the green seed capsule will be found two distinct

series of colorific glands; one yellow, soluble in alcohol, the other purple, soluble in water; the brown is probably made up of these two.

"On the Graphical Representation of the Movements of the Chest-Wall in Respiration," by Arthur Ransome, M.D., M.A.

A stethograph has been constructed which gives an accurate tracing of the course described by any point on the chest-wall, in the forward and upward directions, *i.e.* in the vertical plane at right angles to the anterior surface of the chest.

With this instrument many tracings have been made both in health and disease. The following, chiefly from the anterior ends of the third ribs, are selected as specimens of the results obtained, and to illustrate the conclusions drawn from them.

It has been observed—

1. That the anterior end of the rib takes a different course in its ascent from that of its expiratory descent. (Figs. 1 and 2.)

2. That in most cases the uppermost line is that of inspiration.

3. That this course is liable to variation in consequence of the action of the will. (Figs. 3 and 4.)

4. That in the action of *coughing*, after the inspiratory stroke, there is a slight forward bulging

of the rib, perhaps from the compression of the air in the lungs by the action of the Diaphragm, then a downward fall for a space of about 0·2 in., and afterwards an almost horizontal in-drawing of the rib (Fig. 5) quite unlike anything that could be produced by the simple angular movement of the rib.

5. That in *sneezing* the course of the rib is similar to that taken in the act of coughing, except that there is no forward bulging at the commencement of expiration (Fig. 6), and in the latter part of its course the rib only drops about 0·15 in. for 0·8 in. of in-drawing.

6. That in the dead subject, the movement of the end of the ribs, when simply raised and depressed (fig. 7), approximates to a segment of a circle.

7. That as age advances there is an approach to the form of curve traced by the unyielding rib, and the upward and downward strokes are more nearly alike.

From the above facts it follows that the horizontal in-drawing of the rib, in the spasmodic actions of coughing and sneezing, is impossible without an inbending of the rib itself, and that the variations in healthy breathing are chiefly to be ascribed to the influence of the muscles of forced expiration, which produce more or less bending of the rib during their action.

In disease a comparative feebleness of the respiratory track is to be noticed, and the want of elasticity of the chest is evidenced by the tendency to similarity in the upward and downward course of the ribs.

In acute Phthisis there is a degree of tremulousness in the tracings, and in Pleurisy is seen the effect of adhesions in the very small extent of forward push on the affected side.

The tracing of the cough of Phthisis is, however, similar in its form to that of the healthy chest (fig. 5) though much smaller and more feeble.

Ordinary Meeting, January 27th, 1874.

R. ANGUS SMITH, Ph.D., F.R.S., &c., Vice-President, in the Chair.

Mr. John Watts, Ph.D., was elected an Ordinary Member of the Society.

"On a Source of Error in Mercurial Thermometers," by THOMAS M. MORGAN, Student in the Laboratory of Owens College.

While engaged in distillation, a fact has come under my observation which, although it has been noticed before, does not appear to be very generally known, and has not so far as I have seen been recorded.

The thermometer, which was placed in a Wurtz tube so that the column of mercury was entirely surrounded by the vapour of the distilling liquid, was found after some days to indicate three degrees too little—a discrepancy caused by volatilization from the surface of the column of mercury and condensation on the upper part of the tube. By causing the mercury to flow to the end of the tube and back, the condensed portion was gathered up and the correct temperature indicated. It has since been observed that after each day of distillation, with liquids boiling between 60° and 100° C., a quantity of mercury equal to 1° or 1°·5 volatilizes, and that this quantity is scarcely perceptible when condensed on the surface of the bore. The thermometer in use was about the ordinary size with a scale of 360°.

I am informed that Geissler sometimes encloses a little hydrogen in his thermometers in order that volatilization may not go on so rapidly.

"Notes on fossil Lithothamnia (so-called Nulliporæ)," by ARTHUR WM. WATERS, F.G.S.

The organisms on the table before us have been assigned very various places in the organic or inorganic kingdoms at different times. The recent ones have perhaps most often been placed among the corals, while the fossil forms have passed more frequently as concretionary.

After shortly reviewing the opinions of some of the leading naturalists of this century, Mr. Waters drew attention to a paper of Rosanoff, published in 1866,* on the Melobesiacea, which he divides into three sections, the Melobesia, Lithophyllum, and Lithothamnium. In this paper full description is given as to their mode of growth and their reproduction. Since then Gümbel† has published in 1871 an excellent monograph on the fossil forms under the title, "Die sogenannten Nulliporen," in which he establishes 15 species. This paper has been largely used in preparing the present notes.

As most present will know, they attain their greatest development in the the Leithakalk, a meiocene formation which is principally, in some cases almost entirely, composed of this algæ. Unger says he has never seen any which contains less than two thirds. Thus formations of vast extent and many hundred feet thick, perhaps in some places thousands, are so largely composed of this limestone seaweed. But it is in no way confined to the Leithakalk, being also very abundant in the eocene, especially the upper division; the so-called granit-marmor, or Bavarian marble, a numulitic formation, is very largely composed of this concretionary-looking body. In North Italy it abounds in the eocene formations which are so largely developed in the Veronese and Vicentin. In many places the formation is some hundred feet, much more than half composed of the

* Mem. de la Soc. Imp. des Sc. nat. de Cherbourg, t. xii.

† Abhand. d. R. Bay Ak. der Wissen, x., part i., 1871.

Lithothamnium. It occurs abundantly in Hungary and Switzerland. The so-called pisolithic limestone of Paris is according to Gümbel about eight tenths stone algæ; also M. Mario, Astrup; the pleiocene of Castel Arquato; and in fact it seems to be found in most of the tertiaries on the continent. It is further found in the chalk at Maestricht, and in the jurassic sponge beds at Schwabenbergs.

Doubtless when geologists have paid more attention to the question we shall find it has a very wide distribution, and has formed rocks of equal importance in other parts of the world.

Taking the Eastern Alps, with which I have a fair acquaintance, viz., the Bavarian, Austrian, and North Italian Alps, in which, as you know, the tertiary deposits attain extraordinary importance, I do not think it is too much to say that one sixth of the whole tertiary rocks of this district are composed of Lithothamnium. This is not given in any way as an exact calculation, but merely to give an idea of its importance.

The recent Melobesiacea seem to have a very general distribution. They grow at but moderate depths.

The Lithothamnium belongs to the family Melobesiacea of the order Florideæ or Rhodospermeæ; a large number of this order are limesecreting, but these (Lith.) differ very materially from the Corallina, since in the former the carbonate of lime is deposited in and between the cell structure, while with the Corallina the plant is filiform, and the lime is deposited round this, entirely encrusting it. It seems the structural difference is sufficient to separate them into two entirely distinct classes.

Gümbel, after saying how the stems and branches of a species are subject to variation as well as the superficial characteristics, says, " But by far the most important and numerous species can scarcely be distinguished by any other method than by the form and relative size of the cells,

which can only be made out by transparent sections." We must be very careful how we accept this, for in a piece which I prepared from Torbole, on the Lake of Garda, a drawing of which is shown, we find the same plant producing distinctly three sizes of cells at right angles to one another, in the proportion 5 : 3 : 2.

According to Rosanaff the form of the young is always orbicular.

Gümbel gives the average of various analyses of the Lithothamnium nodosum from Vienna and M. Mario.

Lime	47·14
Magnesia	2·66
Alumina and Oxide of Manganese	2·55
Phosphoric Acid	0·06
Carbonic Acid	40·06
Insol. in Acid	4·96
Water and Loss	2·57
	100·00

The chief point to notice is the large amount of magnesia, representing 5½ per cent carbonate of magnesia. Gümbel points out the important bearing the large amount of magnesia which plants and animals can take up from the water may have upon the question of the formation of dolomitic rocks.

The fossil Lithothamnia are often much altered in colour. One rock on the Lake of Garda might almost be described as a black rock with white spots; an infiltration having taken place from the outside, turning all but the central portion a dark color, so that each piece when broken through shows the centre white, the rest dark. I bring this before your notice to show the great care that is required when judging from lithological characteristics, as no one would at first sight, and often not without microscopical examination, consider such a pure white rock and one almost black to be composed of the same material

In conclusion, the object of this paper is to draw attention to the great masses of these bodies and the importance of always noticing their occurrence in geological formations, since it should be a very material help in regard to the climate, and the conditions of the coasts and currents, besides being of great stratigraphical assistance; nor is it of less importance to note carefully the growth of recent ones, for only through a knowledge of the present can we interpret the past.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 8th, 1873.

CHARLES BAILEY, Esq., Vice-President of the Section, in the Chair.

Mr. R. D. DARBISHIRE, F.G.S., exhibited a collection of shells and fragments of shells, lent for this purpose by Miss M. H. Farington, of Worden Hall. The collection was in itself one of remarkable extent and beauty, as a representative of the fossil fauna of the Drift, and had lately been the subject of a Note read at a meeting of the Geological Society of London. The specimens were found in the Worden gravel pit near the Leyland Station, near Chorley, in a bed of shingle 240 feet above the sea level, shewed 42 species. Amongst these one, Fusus craticulatus, was decidedly Arctic, and the only form of that character. Cytherea chione, and Cardium rusticum were Southern, as was also Mactra glauca. These latter connected the list with that of the Macclesfield Cemetery beds. Fusus propinquus and Fusus antiquus, var. contrarius, which latter appeared here for the first time in English lists of this date, are found in the so-called manure gravels of Wexford.

He also exhibited similar collections made by J. Millard Reade, Esq., F.G.S., in the cuttings of the railway near Edgehill, and in cuttings in Toxteth park, both near Liverpool, 39 species at the former and 29 at the latter place. The Edgehill list includes, like the Leyland list, Saxicava norvegica, Cytherea chione, and Cardium rusticum.

The two latter species occurred in comparison with other species "frequently"; and had now been noted at Edgehill

in "glacial" clay and at Leyland and Macclesfield in gravels. Neither of them had yet been recognised in the Blackpool beds, from which also a small collection was exhibited.

January 19th, 1874.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. PERCIVAL exhibited specimens in fruit of *Hypnum heterophyllum*, gathered this month on wet rocks, Turton, near Bolton, a species which has very rarely been found in fructification.

Mr. SIDEBOTHAM exhibited a specimen of silicious nummulitic rock, from the banks of the Euphrates, the polished section of which exhibited the structure of the shells in a remarkable manner.

JOSEPH SIDEBOTHAM, F.R.A.S., read a paper on "The similarity of certain Crystallised substances to Vegetable forms."

The author alluded to the many well known dendritic markings in shale and slate, which are often mistaken for sea-weeds and mosses, and to the leaf-like forms in various crystallisations. He then called special attention to some experiments carried on by the late Mr. Petschler and himself, on the crystallisation of bichromate of ammonia, nitrate of silver, and other salts, when in combination with gelatine and other colloid substances. The results of these experiments were brought before this society in the year 1861, but so far as he was aware the subject had not been pursued further.

The author then called attention to the formation of verdegris on insect pins, in old Entomological collections. This substance makes its appearance where the pins pass through the thorax of the insects, and in length of time grows into a considerable mass of flocculent matter, of a brilliant green colour, and often breaks up the insects and also destroys the pins. It consists mainly of acetate or formiate of copper in combination with fatty or oily matter.

On examination of various specimens under the microscope, they were found to present a great variety of forms, filamentous and ribbon-like structure, often resembling various fungi, in some cases so nearly, that it was difficult to believe that the fibres and fruit-like forms are not really organic bodies.

Drawings, and specimens under the microscope, were exhibited, and the author expressed his opinion that these bodies were simply crystals, modified in their formation by the oil contained in the insects, with which the crystals is in some way combined. Some of the specimens exhibited were taken from insects collected twenty-five years ago.

An interesting discussion followed the paper, in which the Chairman, Mr. Plant, Mr. Rogers, and other members, took part.

PHYSICAL AND MATHEMATICAL SECTION.

October 14th, 1873.

E. W. BINNEY, F.R.S., F.G.S., in the Chair.

Mr. Samuel Broughton was elected Treasurer of the Section, in place of the late Mr. Thomas Carrick.

"Mean Monthly Barometric Readings at Old Trafford, Manchester, from 1849 to 1872," by G. V. VERNON, F.R.A.S., F.M.S.

Having a tolerably complete register of barometric readings, and knowing of no published normal values for any station in or near Manchester, I have reduced and tabulated the monthly means for the period above named.

The barometer with which the observations were made was a standard by Negretti and Zambra, No. 266, and which had been compared at Greenwich.

All the observations have been corrected for capillarity and index error to reduce them to the Greenwich standard, and then reduced to 32° F.

In the earlier part of the series I have unfortunately some months deficient, but in the latter part of the series I have been able to fill up the gaps by the use of the very careful series of observations made at Eccles by my friend Thomas Mackereth, Esq., F.R.A.S., by applying corrections determined by comparing the months in which the observations were simultaneous.

I found that the two series of observations were nearly identical after allowing for difference of level.

The mean annual pressure for the entire series was 29 780 inches, which, corrected for an altitude of 123 feet above the mean sea level, and assuming the mean annual tempera-

ture to be 50° F., gives us 29·912 inches as the mean yearly value reduced to sea level.

If we take the complete period of the last eleven years we have a mean reading of 29·838 inches. which is 0·074 inches below the value given for the entire period 1849 to 1872.

In the earlier series the month of August was generally wanting in the observations except for a few years, but this month having a reading generally in excess of the yearly value would tend if anything to make the annual value somewhat higher, and the result would tend to show that during recent years the pressure has been lower than during the earlier part of the series.

The variation between the highest and lowest mean yearly readings during the last eleven years is 29·828 inches (1870) and 29·624 inches (1872), or equal to 0·204 inches: this would appear greater than probability would point out, 1872 having had an exceptionally low mean reading; leaving 1872 out, we have a difference of 0·118 inches between 1870 and 1866.

Looking at the mean monthly values, we find that the maximum occurs in June and the minimum in January; the order beginning with the minimum is January, October, March, December, November, September, February, July and April equal, also May and August equal. If fine weather depends upon barometric pressure, the above shows that May and August should be the finest months as a rule, and January and October the worst: May is next to the driest month, April, and October bears off the palm for being the wettest, at least in this district.

The remarkable year for rain, 1872, would appear to have been also remarkable for deficient atmospheric pressure, as the table annexed will show, every month except August having had a pressure below the average, and August was only a few thousandths of an inch above the average.

1872.	Difference from Mean. Inches.
January	−0·315
February	−0·205
March	−0·139
April	−0·032
May	−0·050
June	−0·107
July	−0·017
August	+0·006
September	−0·161
October	−0·195
November	−0·291
December	−0·367

Comparison of these departures below the average with the separate monthly values of the rainfall for 1872 does not show that the greatest rainfall occurred in those months in which the pressure was the most below the average, but the almost constant depression throughout the year certainly does so. It is probable that if a longer and more complete register could be examined, that 1872 had a lower mean pressure than any year over a very long period, just as it had a greater rainfall than any year for a very long period, say 80 years.

BAROMETER MEANS AT 32° F., CORRECTED FOR INDEX ERROR, BUT STILL REQUIRING THE REDUCTION TO THE LEVEL OF THE SEA.

HEIGHT OF BAROMETER CISTERN 123 FEET ABOVE MEAN SEA LEVEL.

"Results of Meteorological Observations taken at Langdale, Dimbula, Ceylon, during the years 1868–72," by Edward Heelis, Esq. Communicated by Joseph Baxendell, F.R.A.S.

The approximate situation of the place of observation is 6° 57′ N.; 79° 42′ E.; and elevation 4,600 feet above sea level.

Mean Temperature.

	1869.	1870.	1871.	1872.	Mean.
January	...	63·78	64·13	65·52	64·48
February	...	65·27	64·79	65·88	65·31
March	...	67·26	66·84	66·84	66·98
April	...	68·36	68·14	67·73	68·08
May	...	68·33	67·65	69·03	68·34
June	...	66·15	64·66	66·55	65·78
July	...	64·48	62·99	65·78	64·42
August	64·02	64·73	65·55	65·40	64·92
September	64·55	64·07	64·12	64·66	64·35
October	64·25	63·85	65·21	64·63	64·48
November	64·59	64·08	65·77	65·18	64·90
December	65·06	64·32	65·64	65·34	65·09
Means......		65·39	65·45	66·04	65·59

Mean Daily Range of Temperature.

	1869.	1870.	1871.	1872.	Mean.
January	...	15·73	14·94	17·49	16·05
February	...	20·39	21·89	19·24	20·51
March	...	21·19	18·62	23·05	20·95
April	...	21·49	18·15	15·80	18·48
May	...	14·56	12·76	16·71	14·68
June	...	11·44	7·45	9·56	9·48
July	...	9·55	7·08	10·12	8·92
August	8·95	10·63	11·10	9·43	10·03
September	9·29	9·81	11·55	8·74	9·85
October	12·46	11·07	15·70	9·52	12·19
November	13·95	17·27	13·41	13·13	14·44
December	17·15	16·43	15·53	14·52	15·91
Means......		14·96	14·01	13·94	14·29

Rainfall.

	1868.	1869.	1870.	1871.	1872.	Mean.	Mean No. of Rainy Days.	Greatest Fall in 24 hours.
	in.	in.	in.	in.	in.	in.		in.
January	...	2·48	10·03	8·98	0·65	5·53	13	2·48
February	...	1·30	2·15	1·40	1·34	1·55	7	1·07
March	...	1·49	2·43	2·12	0·93	1·74	11	1·30
April............	...	7·94	2·93	6·01	9·46	6·58	15	2·96
May	4·68	4·93	3·46	7·85	5·34	5·25	17	2·17
June	11·60	20·02	11·44	21·67	15·64	16·07	25	5·10
July	9·27	18·19	10·52	34·05	8·69	16·14	25	3·77
August.........	5·53	10·54	8·79	11·04	9·54	9·09	24	1·50
September ...	13·52	14·38	16·99	9·17	24·29	15·67	25	5·85
October.........	8·57	17·92	16·62	9·87	12·85	13·16	24	3·14
November ...	4·56	9·17	6·85	8·56	10·42	7·91	20	2·06
December......	6·55	6·37	2·48	2·55	3·58	4·31	15	1·42
Sums.........		114·73	94·69	123·27	102·73	103·00	221	

Difference between Mean Maximum Temperature in the Sun and Mean Maximum Temperature in the Shade.

	1871.	1872.	Mean.
	°	°	°
January	...	27·4	27·4
February	...	31·1	31·1
March	...	27·6	27·6
April............	...	28·8	28·8
May	31·6	25·4	28·5
June	18·6	15·6	17·1
July	17·0	15·7	16·3
August.........	25·6	14·8	20·2
September ...	24·4	14·5	19·4
October.........	29·2	21·8	25·5
November ...	24·6	30·9	27·7
December......	30·0	27·6	28·8

In a letter dated 22nd June, 1873, Mr. Heelis says, "The monsoon came in this year some 10 days before its usual time, a very unusual circumstance, as in the six previous years I have never known it to vary more than 2 days before or after the 1st of June. This year it burst on the 23rd of May, as far as I could determine, but so mildly that for some weeks I doubted its being really the monsoon. Since this date we have not had a single day without rain."

Erratum.—In the abstract of Dr. Ransome's paper "On the Graphical Representation of the Movements of the Chest-wall in Respiration," given in the last number of the Proceedings, the references to three of the figures are erroneous. The figure representing the act of coughing is No. 6, that of sneezing No. 7, and the motion of the dead rib is given in fig. 5.

Ordinary Meeting, February 10th, 1874.

R. Angus Smith, Ph.D., F.R.S., &c., Vice-President, in the Chair.

"The Northern Range of the Basques," by W. Boyd Dawkins, M.A., F.R.S., F.S.A.

The northern extension of the Basque race from their present boundary, in ancient times, is demonstrated by the convergent testimony of history, ethnology, and the researches into caves and tombs.

The Evidence of History as to the Peoples of Gaul and Spain.

In the Iberian peninsula the Basque populations (Vascones) of the west are defined from the Celtic of the east by the Celtiberi inhabiting modern Castille. In Gaul the province of Aquitania extended as far north, in Cæsar's time, as the river Garonne, constituting the modern Gascony, to which was added, in the days of Augustus, the district between that river and the Loire, a change of frontier that was probably due to the predominence of Basque blood in a mixed race in that area similar to the Celtiberi of Castille. The Aquitani were surrounded on every side, except the south, by the Celtæ, extending as far north as the Seine, as far to the east as Switzerland and the plains of Lombardy, and southwards, through the valley of the Rhone and the region of the Volscæ, over the Eastern Pyrenees into Spain. The district round the Phocæan colony of Marseilles was inhabited by Ligurian tribes, who held the region between the river Po and the Gulf of Genoa, as far as the western boun-

dary of Etruria, and who probably extended to the west along the coast of Southern Gaul as far as the Pyrenees. They were distinguished from the Celtæ, not merely by their manners and customs, but by their small stature and dark hair and eyes, and are stated by Pliny and Strabo to have inhabited Spain. They have also left marks of their presence in Central Gaul in the name of the Loire (Ligur), and possibly in Britain in the obscure name of the Lloegrians. Their stature and swarthy complexion, as well as the ancient geographical position conterminous with the Iberic population of Gaul and Spain, confirm this conclusion. The non-Aryan and probably Basque population of Gaul was therefore cut into two portions by a broad band of Celts, which crosses the Eastern Pyrenees, and marks the route by which the Iberian peninsula was invaded.

The ancient population of Sardinia is stated by Pausanias to be of Libyan extraction, and to bear a strong resemblance to the Iberians in physique and in habits of life, while that of Corsica is described by Seneca as Ligurian and Iberian. The ancient Libyans are represented at the present day by the Berber and Kabyle tribes which are, if not identical with, at all events cognate with the Basques. We may therefore infer that these two islands were formerly occupied by this non-Aryan race, as well as the adjacent continents of Northern Africa and Southern Europe.

The Basque Population the Oldest.

The relative antiquity of these two races in Europe may be arrived at by this distribution. The Basques, or Ligurians, are the oldest inhabitants, in their respective districts, known to the historian; while the Celts appear as invaders, pressing southwards and westwards on the populations already in possession, flooding over the Alps, and, under Brennus, sacking Rome, and by their union with the vanquished in Spain constituting the Celtiberi. We may

therefore be tolerably certain that the Basques held France and Spain before the invasion of the Celts, and that the non-Aryan peoples were cut asunder, and certain parts of them left — Ligurians, Sikani, and in part Sardinians and Corsicans — as ethnological islands, marking, so to speak an ancient Basque non-Aryan continent which had been submerged by the Celtic populations advancing steadily westwards.

At the time of the Roman conquest of Gaul, the Belgæ were pressing on the Celts just as the latter pressed the Basques, the Seine and the Marne forming their southern boundary, and in their turn being pushed to the east by the advance of the Germans in the Rhine provinces. Thus we have the oldest population, or Basque, invaded by the Celts, the Celts by the Belgæ, and these again by the Germans; their relative positions stamping their relative antiquity in Europe.

The Population of Britain.

The Celtic and Belgic invasion of Gaul repeated itself, as might be expected, in Britain. Just as the Celts pushed back the Iberian population of Gaul as far south as Aquitania, and swept round it into Spain, so they crossed over the Channel and overran the greater portion of Britain, until the Silures, identified by Tacitus with the Iberians, were left only in those fastnesses that formed subsequently a bulwark for the Brit-Welsh against the English invaders. And just as the Belgæ pressed on the rear of the Celts as far as the Seine, so they followed them into Britain, and took possession of the "Pars Maritima," or southern counties. The unsettled condition of the country at the time of Cæsar's invasion was, probably, due to the struggle then going on between the Celts and Belgæ.

Basque Element in present British and French Populations.

The Basque non-Aryan blood is still to be traced in the dark-haired, black-eyed, small, oval-featured peoples in our own country in the region of the Silures, where the hills have afforded shelter to the Basque populations from the invaders. The small swarthy Welshman of Denbighshire is in every respect, except dress and language, identical with the Basque peasant of the Western Pyrenees, at Bagnères de Bigorre.

The small dark-haired people of Ireland, and especially those to the west of the Shannon, according to Dr. Thurnam and Professor Huxley, are also of Iberian derivation, and, singularly enough, there is a legendary connection between that island and Spain. The human remains from the chambered tombs as well as the river-beds prove that the non-Aryan population spread over the whole of Ireland as well as the whole of Britain. The main mass of the Irish population is undoubtedly Celtic, crossed with Danish, Norse, and English blood.

The Basque element in the population of France is at the present time centered in the old province of Aquitaine, in which the jet-black hair and eyes, and swarthy complexion, strike the eye of the traveller, now, as in the days of Strabo, and form a vivid contrast with the brown hair and grey eyes of the inhabitants of Celtica and Belgica. The map published by Dr. Broca ("Memoires d'Anthropologie," t. I., p. 330) shows at a glance the average complexion prevailing in each department, and the relative number of exemptions per 1,000 conscripts, on account of their not coming up to the standard of height (1·56 metre = 5 feet 1½ inches), and it will be seen that the only swarthy people outside the boundary of Aquitaine, constitute five ethnólogical islands. Of these Brittany is by far the largest, probably because its fastnesses afforded a shelter to the Basques, who were being

driven to the south-west. The department of the Meuse in the north, and those of Tarn and Arriège, in the south, are also sundered from the main body, while those of the Upper and Lower Alps present us with the descendants of the ancient Ligurian tribes.

The people with dark-brown hair, considered by Dr. Broca to be the result of the intermingling of a dark with a fair race, are scattered about through Aquitaine, and occur only in two departments in northern Celtica. The fair people, on the other hand, are massed in northern Celtica and Belgica.

The relation of complexion to stature may be gathered from the following table of exemptions per 1,000 for each department :—

Départements	noirs	98·5	to	189
„	gris foncés	64	„	97
„	gris clairs	48·8	„	63·8
„	blancs	23	„	48·5

From this table it is evident that the swarthy people are the smallest and the fair the tallest, the intermediate shades being the result of fusion between the two extremes.

The distribution, therefore, of the small swarthy Basque, and tall fair Celtic and Belgic races in France at the present time, corresponds essentially with that which we might have expected from the evidence of history.

When we consider the many invasions of France, and the oscillations to and fro of peoples, the persistence of the Basque population is very remarkable. It is not a little strange that the type should be so slightly altered by intermarriage with the conquering races.

Researches in Neolithic Caves and Tombs.

The evidence offered by an appeal to history and ethnology, as to the former northern extent of the Basque peoples, is confirmed by an examination of the human remains in the

Neolithic caves and tombs, scattered throughout the area under consideration. The discoveries in the caves of Gibraltar and of the Spanish Mainland prove that a small, long-headed race, with delicate features and orthognathic profile identical with the Basques who buried their dead in the modern cemetery of Guipuscoa ranged throughout the Peninsula, using with indifference caves and chambered tumuli for their tombs. And on the same grounds their former range through France, Britain, and Ireland, is demonstrated, and as far to the east as Belgium. They occupied the whole of this region in the Neolithic age, in which they were invaded and driven to the westward, and broken up into islands by the Celts, a fair, tall, broad-headed race. The Basques, therefore, have lived in Europe since the Neolithic age, history, ethnology, and researches in caves and tombs, offering independent and convergent testimony. At the present time the Basque blood asserts itself in the physique of certain isolated populations, and within the historic period is demonstrated to have been more strongly defined, and to have occupied larger areas, and lastly in the prehistoric period to have formed one continuous race from the Pillars of Hercules, as far north as Scotland, and as far to the east as Belgium.

PHYSICAL AND MATHEMATICAL SECTION.

February 3rd, 1874.

ALFRED BROTHERS, F.R.A.S., President of the Section, in the Chair.

"On the Theory of the Tides," by DAVID WINSTANLEY, Esq.

According to that theory of the tides which appears to have met with something like general acceptance, the waters of the ocean are heaped up on opposite sides of the earth by those differences of lunar attraction which result from

inequalities of distance, "the waters being pulled from the earth on the one side and the earth from the waters on the other."

In any of those expositions of this theory which have come under my notice I have failed to observe any mention of that resistance to the lunar attraction without the consideration of which it appears to me that the theory in question must be regarded as incomplete. One of the known results of the mutual gravitation of the earth and moon is found in the instance of the former body in an oscillation of its major portion on both sides of a given line, viz. the orbital path pursued by the centre of gravity of the binary system. The oscillation in question is certainly resisted by the tendency of the earth's particles to continue their motion in right lines, and the amount of this resistance clearly increases with the extent or violence of the oscillation, which attains its maximum at that portion of the earth's surface most remote from the moon, and sinks to zero at the common centre of gravity of the system. Without these differences of resistance to the forced oscillation caused by the lunar influence, it seems to me the differences of lunar attraction would fail to produce the effects ascribed thereto.

If this be so, there can be no doubt as to the desirability of making mention in any exposition of the tidal theory both of the resistance I have mentioned and its differences in extent, and the more especially as we know on the highest modern astronomical authority that "strange difficulties" are experienced by many of those who attempt from the expositions now in use to gain a knowledge of the *modus operandi* of the tides. The tendency to continued rectilinear movement to which I have alluded as a resistance to oscillation is however well known by the name of "centrifugal force," which term, or any of its modern substitutes, may, it seems to me, be conveniently employed in considering the theory of the tides.

If we discard for the time all considerations of how it has come to be so, and regard only the fact that the earth and moon together constitute a system which revolves upon its common centre of gravity, I am of opinion that we shall have unmixed with distracting matter all the facts that are necessary for theorising on the production of the lunar tides. It will be clear that the revolution in question will be accompanied by the development of centrifugal force, and the whole point to which attention is now directed is whether or no the said centrifugal force contributes in any material manner to the phenomena of the tides. It is needless to complicate the matter by any attempt to reduce the value of this centrifugal force to an expression of the motion or pressure of a number of tons or pounds. We know that at the earth's centre it is just sufficient to overcome that tendency to juncture which is the simple resultant of the gravitation subsisting between the earth and moon. Were it greater, the interval of separation would increase; whilst were it less, it would diminish. The constancy of this interval* proves the equality at the earth's centre at any rate of the attraction which perturbs and the centrifugal force which offers resistance to perturbation.

For convenience of expression we may apply the term "lunar unit" to indicate the quantity of either of the forces we have named. It will at once be evident that on the side of the earth nearest the moon there will be experienced the effect of more than a lunar unit of attraction and less than a lunar unit of resistance. Indeed the centrifugal force which at the further side of the earth is opposed to the attraction of the moon here augments the effects produced thereby, so that we have a tide-producing force equal to the sum of the forces in question. On the other side of the earth, however, we have less than a lunar unit of attraction, and more than a lunar unit—in fact more than two lunar

* The writer is not unaware of the ellipticity of the moon's orbit.

units of resistance; so that we have there a tide-producing force due to the resistance alone and equal in amount to the difference between that resistance and the amount of the lunar attraction.

A very little reflection will suffice to show that in whatever part of the earth's figure the common centre of gravity of the system may be found, the tide-producing energy resulting from the forces here named will be substantially the same on both sides of the earth. For instance, if we regard the figures representing the earth's diameter, the moon's distance, and the mass of both bodies, as being correctly given in the tenth edition of Herschel's "Outlines," we shall find the centre of gravity of the system to be 1249 miles below the surface of the earth. Here we have no centrifugal force at all, but at a distance beyond of 2713 miles (*i.e.* at the centre of the earth) we have one lunar unit and no more. Under the moon, where the attraction is 1·034 lunar units, the augmenting centrifugal force is ·46 of a lunar unit, and the tide-producing force equal to the amount of their sum, or 1·494 times the same standard of measure. On the further side of the earth, however, the centrifugal force equals 2·46 times the amount experienced at the centre, whilst the attraction of the moon at the same place is ·968 of that amount. The difference of these forces or 1·492 lunar units indicates the comparative amount of force available for raising the external terrestrial tide.

"Rainfall at Old Trafford, Manchester, in the year 1873," by G. V. VERNON, F.R.A.S., F.M.S.

The rainfall of 1873 was 5·950 inches below the average of the last 80 years, and no less than 21·877 inches below the great fall of 1872.

The total fall in 1873 was 29·815 inches, and fell upon 198 days; although the fall was below the average it fell upon an unusual number of days, and was 6 days above the average of the last twelve years.

January had a rainfall in excess of the average; February, March, April, and May had a fall below the average; June, July, and August were all in excess, and this joined to very little hot weather, excepting a short period in July, spoiled the prospects of the harvest; September had a fall below the average, October was above; November and December had a rainfall greatly below the average of the season, December especially, and excepting April was the driest month of the year.

The rainfall for the first quarter of the year was 1·628 inches below the average; for the second quarter, 1·914 inches below the average; for the third quarter, 0·937 inches above the average; whilst for the fourth quarter it was 3·445 inches below the average.

This distribution of the rainfall was unfortunate as the heaviest amounts fell at the time when the crops generally should have been ready for cutting: fortunately the weather suited the hay crop, which was generally a very good one, and there was plenty of after-grass.

Rain Gauge 3 feet above the ground and 106 feet above sea level.

Quarterly Periods. 1872.	Quarterly Periods. 1873.	1873.	Fall in Inches.	Average of 80 Years.	Difference.	No. of Days Rain fell in 1873.	Quarterly Periods. 80 Years.	Quarterly Periods. 1873.	Quarterly Periods. Differ'nce
			ins.	ins.	ins.		ins.	ins.	ins.
56	48	January..	3·136	2·544	+0·592	21	7·220	5·592	—1·628
		February.	0·666	2·388	—1·722	11			
		March....	1·790	2·288	—0·498	16			
50	43	April......	0·516	2·045	—1·529	12	7·207	5·393	—1·914
		May	1·910	2·297	—0·387	17			
		June	2·967	2·865	+0·102	14			
59	64	July	4·649	3·572	+1·077	20	10·388	11·325	+0·937
		August ...	4·199	3·509	+0·690	27			
		Septemb'r	2·477	3·307	—0·830	17			
63	43	October...	4·441	3·899	+0·542	20	10·950	7·505	—3·445
		November	2·283	3·765	—1·482	13			
		December	0·781	3·286	—2·505	10			
228	198		29·815	35·765	—5·950	198	35·765	29·815	—5·950

Ordinary Meeting, February 24th, 1874.

Rev. WILLIAM GASKELL, M.A., Vice-President, in the Chair.

Mr. A. BROTHERS, F.R.A.S., referring to a statement made by Mr. Dawkins, F.R.S., at the last meeting, that the sinking of a deep well recently at Sheerness had lowered the water in a well at Southend, read the following extract of a letter dated February 21st, 1874, which he had received from Mr. Joseph Beard, of Southend: —

"I have seen the agent of our water works and gleaned what particulars I could. Nothing has occured of recent date affecting our water at Southend; but about fifty years ago a well was sunk in the dockyard at Sheerness which by excessive pumping did drain all the deep wells at Southend. Our well at the water works was sunk fourteen years ago. It is 905 feet deep, dug to 385 feet, the rest a boring. The last 300 feet is through chalk, above that a layer of green sand. The water is particularly soft, and in washing you can't distinguish it from rain water. Nothing from Sheerness has disturbed this well since it was sunk, but the company had an accident at their beginning which may have given rise to what you have heard. They sank their first well 400 feet, and all at once broke into water that rose 200 feet. They tried to pump it out, but could not do so, and at last the bottom gave way and their pumping apparatus slipped down, where it remains unrecoverable at the present moment. They then started their labour again at a distance of 40 feet, but had great trouble with the quicksand through which they had to get to the chalk."

Mr. E. W. BINNEY, F.R.S., F.G.S., stated that of late years much had been said and written on the advantages of drainage in improving the sanitary condition of towns.

Now this, like many other good things, can be carried too far, especially in districts situate on a sandy soil and where the owners of crowded graveyards are permitted to bury corpses near to the public highway, a proceeding occasionally yet allowed by sanitary authorities. Some years since a main sewer was excavated on the Cheetham Hill Road. From the Workhouse to Temple it was made in strong brick clay or "till," but when it reached the latter place it came into a mass of quicksand and drained St. Luke's churchyard and took the water down to Manchester. The sewer was then continued in the sand all the way to the Bird in the Hand public house, where it stopped. Within the last month a main drain has been made from the turnpike road up Robin Hood Street so as to drain the water from part of St. Mark's churchyard. This drain runs into a new main sewer which joins the old one near the Bird in the Hand, and thus the drainage of part of St. Mark's churchyard is made to join that of St. Luke's, and both go down to Manchester. No doubt sand possesses a great filtering power, and will in some measure purify the water drained from churchyards; but water derived from such sources ought to be examined carefully before it is conveyed even into sewers connected with dwelling-houses. Probably our officers of health may have done so, and think that there is no harm in water flowing from graveyards, but as yet, so far as known to me, no information has been given to the public on this subject. The making of sewers in sandy soils near to burying grounds is a matter that deserves the attention of sanitary officers more than it has done up to this time.

Burial grounds cannot well be removed when once established, but their owners can be prevented from burying close to roads and streets having sewers under them, and caution can be used in making new burial grounds near to roads and sewers. When burial grounds adjoin churches,

whatever the consequences, some people will build houses near them, preferring the contiguity of the church to any fear of the evils of a crowded cemetery.

"On the Effect of Acid on the Interior of Iron Wire," by Professor OSBORNE REYNOLDS, M.A.

It will be remembered that at a previous meeting of this Society Mr. Johnson exhibited some iron and steel wire in which he had observed some very singular effects produced by the action of sulphuric acid. In the first place the nature of the wire was changed in a marked manner, for although it was soft charcoal wire it had become short and brittle; the weight of the wire was increased; and what was the most remarkable effect of all was that when the wire was broken and the face of the fracture wetted with the mouth it frothed up as if the water acted as a powerful acid. These effects, however, all passed off if the wire were allowed to remain exposed to the air for some days, and if it were warmed before the fire they passed off in a few hours.

By Mr. Johnson's permission I took possession of one of these pieces of wire and subjected it to a farther examination, and from the result of that examination I was led to what appears to me to be a complete explanation of the phenomena.

I observed that when I broke a short piece from the end of the wire the two faces of the fracture behaved very differently — that on the long piece frothed when wetted and continued to do so for some seconds, while that on the short piece would hardly show any signs of froth at all. This seemed to imply that the gas which caused the froth came from a considerable depth below the surface of the wire, and was not generated on the freshly exposed face. This view was confirmed when on substituting oil for water I found the froth just the same.

These observations led me to conclude that the effect was due to hydrogen, and not to acid as Mr. Johnson appeared to think, having entered into combination with the iron during its immersion in the acid, which hydrogen gradually passed off when the iron was exposed.

It was obvious however that this conclusion was capable of being further tested. It was clearly possible to ascertain whether or not the gas was hydrogen; and whether hydrogen penetrated iron when under the action of acid. With a view to do this I made the following experiments.

First, however, I would mention that after 24 hours I examined what remained of the wire, when I found that all appearance of frothing had vanished and the wire had recovered its ductility, so much so that it would now bend backwards and forwards two or three times without breaking, whereas on the previous evening a single bend had sufficed to break it.

I then obtained a piece of wrought iron gas pipe 6 inches long and $\frac{5}{8}$ inch external diameter, and rather more than $\frac{1}{16}$ of an inch thick; I had this cleaned in a lathe both inside and outside; over one end I soldered a piece of copper so as to stop it, and the other I connected with a piece of glass tube by means of indiarubber tube. I then filled both the glass and iron tubes with olive oil and immersed the iron tube in diluted sulphuric acid which had been mixed for some time and was cold. Under this arrangement any hydrogen which came from the inside of the glass tube must have passed through the iron.

After the iron had been in the acid about 5 minutes small bubbles began to pass up the glass tube. These were caught at the top and were subsequently burnt and proved to be hydrogen. At first, however, they came off but very slowly, and it was several hours before I had collected enough to burn. With a view to increase the speed I changed the acid several times without much effect until I happened to

use some acid which had only just been diluted and was warm; then the gas came off twenty or thirty times as fast as it had previously done. I then put a lamp under the bath and measured the rate at which the gas came off, and I found that when the acid was on the point of boiling as much hydrogen was given off in 5 seconds as had previously come off in 10 minutes, and the rate was maintained in both cases for several hours.

After having been in acid for some time the tube was taken out, well washed with cold water and soap so as to remove all trace of the acid; it was then plunged into a bath of hot water, upon which gas came off so rapidly from both the outside and inside of the tube as to give the appearance of the action of strong acid. This action lasted for some time, but gradually diminished. It could be stopped at any time by the substitution of cold water in place of the hot, and it was renewed again after several hours by again putting the tube in hot water. The volume of hydrogen which was thus given off by the tube after it had been taken out of hot acid was about equal to the volume of the iron.

At the time I made these experiments I was not aware that there had been any previous experiments on the subject; but I subsequently found, on referring to Watt's Dictionary of Chemistry, that Cailletet had in 1868 discovered that hydrogen would pass into an iron vessel immersed iu sulphuric acid. See *Comp. rend.* lxvi, 847.

The facts thus established appear to afford a complete explanation of the effects observed by Mr. Johnson.

In the first place, with regard to the temporary character of the effect, it appears that hydrogen leaves the iron slowly even at ordinary temperatures — so much so that after two or three days' exposure I found no hydrogen given off when the tube was immersed in hot water. With regard to the effect of warming the wire — at the temperature of boiling

the hydrogen passed off 120 times as fast as at the temperature of 60°. Also when the saturated iron was plunged into warm water the gas passed off as if the iron had been plunged into strong acid; so that we can easily understand how the hydrogen would pass off from the wire quickly when warm, although it would take long to do so at the ordinary temperatures. With regard to the frothing of the wire when broken and wetted—this was not due, as at first sight it appeared to be, simply to the exposure of the interior of the wire, but was due to warmth caused in the wire by the act of breaking. This was proved by the fact that the froth appeared on the sides of the wire in the immediate neighbourhood of the fracture, when these were wetted, as well as the end; and by simply bending the wire it could be made to froth at the point where it was bent.

As to the effect on the nature and strength of the iron I cannot add anything to what Mr. Johnson has already observed. The question, however, appears to be one of very considerable importance, both philosophically and in connection with the use of iron in the construction of ships and boilers. If, as is probable, the saturation of iron with hydrogen takes place whenever oxidation goes on in water, then the iron of boilers and ships may at times be changed in character and rendered brittle in the same manner as Mr. Johnson's wire, and this, whether it can be prevented or not, is at least an important point to know, and would repay a further investigation of the subject.

Dr. Ransome, M.A., demonstrated the movements of the chest in respiration, showing the remarkable mobility of its several parts, and the consequent facility with which its cavity can be inflated.

Its motions are conditioned by the shape and mode of articulation and degree of movement of the bones composing it.

The motions of points on its anterior wall on either side of the breast bone may take place in an upward, forward, and lateral direction, and a stethometer has been constructed by means of which these movements may be measured during any one act of breathing, in three planes at right angles to one another.

From the records obtained with this instrument it has been observed that the ratio of the forward to the upward and outward movements varies very greatly, not only in different individuals but in the same person, and that by the constraining influence of the will it is possible to cause at one time the forward and at another the upward motion to predominate.

The upper ribs have sometimes more forward movement than the lower; in childhood its extent is very large, but with the advance of age the movement in this plane becomes comparatively very small. In disease the ratios of the motions in the three planes are much altered—especially in the early stages of consumption and in pleurisy after the effusion has been absorbed.

By means of an instrument for ascertaining the angles made by the plane of the rib-circuits with the vertical it may be demonstrated mathematically—

1. That the upward dimensions of the movements of the anterior ends of the ribs are sufficiently accounted for by the upward rise of the ribs, their chordlengths being taken as radii, their vertebral attachments as centres.

2. The outward indications are also probably to be accounted for by the radial rise of the costal cartilages, the sternal articulations being taken as centres.

3. But from the above-mentioned observations it is obvious that the forward thrust of the anterior ends of the ribs cannot be accounted for by their simple rise from a more to a less oblique position. From the records of the chest movements it appears that there is no constant relation to be

found between the amounts of forward and upward movements, and from the measurement of the angles made by the ribs with the spine it may be shown, that these angles are not such as to permit of the degree of forward motion recorded by the three-plane stethometer.

In some instances the discrepancy between the observed and the calculated forward movement amounted to 0·5in. for the fifth rib and 0·7in. for the third rib.

From these facts the conclusion has been drawn that in forced breathing the extreme effort of expiration causes a certain degree of inbending of the ribs.

This explanation was further shown to be correct by the use of a pair of callipers, especially devised for measuring the diameters of the rib-circuits in the two positions of extreme inspiration and forced expiration. In the case exhibited to the Society the difference between the chordlength of the right third rib in expiration and inspiration was proved to be about 0·4in.

Further confirmation of these views was found in the tracings of the stethograph, shown to the Society four weeks ago.

Diagrams of these tracings, prepared by the Rev. Brooke Herford, were exhibited to the meeting.

Mr. CARSON desires to correct a statement which appears in a notice read at the meeting on January 13th last, "On a Crystalline Sublimed Cupric Chloride," in which, through a misunderstanding, he stated that sodium chloride was volatilized in Deacon's chlorine process when sodium sulphate is added to the copper salt. He since has learnt from Mr. Deacon that no such volatilization of sodium chloride has been observed.

Ordinary Meeting, March 10th, 1874.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

The CHAIRMAN said that at a meeting of the Society on the 9th day of January, 1872, in presenting to the notice of the members specimens of fossil woods from the lower coal measures of Lancashire, he stated "that from some examples in his cabinet he was led to believe that Cotta's *Medullosa elegans* was merely the rachis of a fern or a plant allied to one." Now, Professor Renault, of Paris, to whom we owe so much for his researches in fossil botany, has lately read a memoir before the French Academy on the 26th January last, which has since been printed in the *Comptes Rendus*, that completely confirms this opinion.

The genus *Medullosa* was first given by Professor Cotta to some specimens of fossil plants found at Chemnitz. M. Brongniart changed the genus into *Myeloxylon*, and M. Renault has now altered it into *Myelopteris* and made two species, *M. radiata* and *M. Landriotti*.

The fossil is found in great abundance in the calcareous nodules of the Upper Brooksbottom coal in Lancashire, and varies in size from one tenth of an inch to an inch in diameter. In this district no leaves of ferns have been found attached to it, but in the strata adjoining the seam of coal where the nodules occur specimens of *Neuropteris*, with other ferns, have been met with. Professor Renault states that M. Grand' Eury refers the petioles of *Myelopteris* to *Neuropterides*, which comprehend the *Neuropteris*, the *Odontopteris*, &c. Side by side in the same nodules the most common plant found with *Myelopteris* is *Calamodendron commune* and the small cone which from similarity or structure has been supposed by me to be the fructification of that plant.

Professor Renault has been so fortunate as to have obtained a fine collection of specimens in a most perfect state of preservation from Autun, and they could scarcely have fallen into better hands. After describing them at length he comes to the following conclusion:

"D'après ce qui précède, il est donc à peu près certain que ces pétioles de *Myelopteris* sont des pétioles de Fougères, ayant eu le mode de croissance et le port actuel de nos *Angiopteris*, dont ils différaient pourtant à certains égards, et l'on peut les considérer comme ayant formé un genre d'une grande importance à l'epoque carbonifere, mais actuellement complétement éteint, que l'on doit ranger dans la famille de Marattiees."

"Further Observations and Experiments on the Influence of Acids on Iron and Steel," by WILLIAM H. JOHNSON, B.Sc.

At the last meeting of the Society Professor Reynolds in an interesting paper "On the Effect of Acid on the Interior of Iron Wire," appears to think that I did not attribute to hydrogen any portion of the remarkable change produced in iron and steel by immersion in acid. That immersion in acid is the primary cause no one, I think, will dispute; but that hydrogen plays an important part in producing these changes and is the cause of the bubbles, the following paragraph from a paper I read before the Society, March 4th, 1873, will prove:

"The experiments of Professor Graham in 1867, and more recently those of Mr. Parry, show that hydrogen, carbonic oxide and carbonic acid, and nitrogen, are evolved from wrought iron, cast iron, and steel, when heated in vacuo. Therefore it seems probable that a part of the hydrogen produced by the action of the acid on the iron may be absorbed by the iron, its nascent state facilitating this. And when the iron is heated by the effort of breaking it, the gas may bubble up through the moisture on the fracture."

The supposition that the absorption of hydrogen is the sole cause of the change in the breaking strain, diminution in toughness, etc., attendant on the immersion of iron in hydrochloric or sulphuric acids, and that there is no absorption of these acids into the interior of the iron, does not account for the following phenomena :—

1.—The gain in weight of a piece of iron by immersion in hydrochloric acid is less than by immersion in sulphuric acid, as is proved by experiments described in my first paper on this subject.

2.—Iron after immersion in hydrochloric acid sooner regains its original state than after immersion in sulphuric acid.

3.—If acid iron, *i.e.*, iron which has been immersed in hydrochloric or sulphuric acids, be steeped in an alkaline solution it sooner regains its original state than with immersion in water alone.

4.—Take two pieces of iron alike in size and quality, and immerse one in hydrochloric acid and the other in sulphuric acid for some hours, then wash them well in water and dry them gently, and leave them in a temperate room for some hours more. At the end of that time it will be invariably found—that the piece which was in hydrochloric acid is covered with a dark-brown red oxide of iron, while the piece which was in sulphuric acid will be only slightly rusted.

5.—Litmus paper when applied to the moistened fracture of acid iron is slightly reddened.

All the above phenomena have been observed so often and so carefully as to leave no doubt of their invariable recurrence if the conditions of experiment be only properly observed.

It seems to me the only satisfactory way of explaining all the phenomena is to suppose that when a piece of iron is immersed in acid two actions go on, viz.; An absorption

of the nascent hydrogen into the interior of the iron, which hydrogen may subsequently be given off by gentle heat or immersion in a liquid, etc. Secondly, an absorption of the acid itself, possibly in a very concentrated form by the interstices between the fibres or crystals of the metal.

That it is possible for a liquid to pass into the interior of a piece of iron is I think proved by the sweating of the cylinders of hydraulic presses, and also the known diffusion of gases through iron. The structure of iron as revealed by the microscope and the changes it undergoes during manufacture, by which a spongy mass is by hammering and rolling squeezed together, all go to prove that there are numerous cavities in iron and steel.

It will however be said, the acid must act on the walls of the cavity and form a salt of iron with liberation of hydrogen. This may go on to a small extent, but in opposition to this view we may bring the experiments of Prof. Bequerel on solutions separated by a cracked tube (*Comptes Rendus*, LXXVI), where he shows that no precipitate is formed on placing a cracked tube filled with nitrate of lead in a solution of potassium sulphate within the crack, thus making it probable that chemical interchanges do not take place in very minute spaces.

By this theory we may easily explain the decrease in toughness after immersion in acid. For toughness implies a certain ease of mobility of the particles. When a piece of iron is bent the particles of one side are compressed, thus diminishing the minute cavities between the fibres, while those of the other side are stretched, and the minute cavities elongated. Now if we fill these cavities with a liquid this mobility of the particles is prevented, for the cavities cannot now be diminished in size and the compression of the one side cannot now take place, consequently the piece tears or breaks off just like a piece of frozen rope.

It will also explain the acid reaction of the moistened

fracture, and furthur, as hydrochloric acid is much more volatile and of less specific gravity than sulphuric acid, it is only natural to expect that the effect of immersion in hydrochloric acid will pass off more rapidly than of immersion in sulphuric. This experience fully confirms.

Influence of Immersion in Acid on the Tensile Strain and Elongation.

With a view of determining these interesting points a number of experiments were made in the following way, viz.: Small coils of iron wire were immersed in hydrochloric and sulphuric acids for different lengths of time, and then carefully tested for tensile strain in a very accurate machine, so constructed that the elongation of the wire while under strain could at any moment be ascertained. The weights could also be added quickly and without imparting any shock to the wires, points to which great importance should always be attached in experiments of this kind, as a slight shock or jar on the addition of a weight will often cause the rupture of a piece which otherwise would have stood a much higher strain. The length of the pieces tested was in all the experiments 10 inches between the dies of the machine, and their temperature at the time of experiment about 16° C.

After the ultimate elongation and breaking weight had been ascertained with this machine, the coils were placed on warm plates or in hot chambers for some hours and subsequently tested in the same way.

Experiments, the results of which are shown in tables A, B, C, were made in this way, and lead us to the following conclusions:

1st. That immersion in hydrochloric acid for 1 hour diminishes the

Tensile strain of annealed iron 297lbs. per sq. inch of section.

„ unannealed iron 2,389lbs. „ „

and diminishes the

Ultimate elongation of annealed iron 0·8 per cent.

„ unannealed iron 0·83 „

2nd. That immersion in hydrochloric acid for 6 hours diminishes the

Tensile strain of annealed mild steel 2,563lbs. per sq. in. of section.

and increases the

Ultimate elongation of annealed mild steel 4·7 per cent.

When first I discovered that a decrease of ultimate elongation under strain was the result of immersion of steel in acid, I thought there must be some experimental error, and accordingly carefully selected 3 coils, all of uniform temper, and made 3 tests from each coil, the results of which are given in detail in table C. The singular regularity of these tests must be said to remove all doubt as to the truth of the results of the first experiment.

It then occurred to me that possibly *prolonged* immersion in acid might so decrease the breaking strain that the wire would not recover its original strength. For this purpose I carefully tested the elongation and breaking strain of the wire before immersion in acid and again after heating for 5 days on a hot plate. The results of these experiments as given in tables D and E confirm this view, showing that there is

1st. A permanent decrease in breaking strain after prolonged immersion in

Sulphuric acid of 12,205lbs. per sq. inch of section.

Hydrochloric „ 31,275lbs. „ „

Both these results are doubtless too high as the surface of the wire was pitted by the acid, consequently its actual sectional area was less than that calculated from the diameter as measured.

2nd. A permanent increase in the elongation after prolonged immersion in

Sulphuric acid of 1·7 per cent.

Hydrochloric „ 2·17 „

Having examined the effect of acid on *annealed* steel it was next thought advisable to try the effect on *unannealed*. This was done with results as in table F, showing that the

immediate effect of immersion in acid was to

Decrease the tensile strain of unannealed steel 4,045lbs. per square inch of section.

And increase the ultimate elongation of „ „ 1·14 per cent.

The change is thus similar to that which takes place in annealed steel as shown in table C. It is, however, interesting to observe that 12 hours at a temperature of 40°—100° C. not only restores but actually increases its original breaking strain and elongation, while a still more prolonged submersion of 7 days to the same temperature still further increases them. These last experiments also show that some considerable time is required to overcome the change produced by the acid.

In conclusion I may say that the numerical results arrived at, though based on experiments conducted with considerable care, must not be taken as more than approximations to the truth, for experimental errors and variations arising from the imperfect homogeneity of structure of all iron falsify the results and are only lost by multiplying experiments almost indefinitely.

Table A.

Effect of Hydrochloric Acid on Annealed Iron Wire.

Description.	Diametr inches.	Ultim'te Elongation.	Increase in ditto.	Breaking strain.	Breaking strain per sq. inch of section.	Increase in ditto.	No. of tests of wh. the mean is given.
				lbs.	lbs.	lbs.	
Annealed Iron Wire immersed in acid 1 hour...	·150″	22·4°/o		918·4	51,890		9
Same piece as above after being 48 hrs. on a hot plate of a temperature of 40°-200° C......	·150″	22·3°/o	—0·1°/o	922·8	52,140	250	9
Annealed Iron Wire immersed in acid 1 hour...	164″	18·6°/o		1105·3	52,389		3
Same piece as above being 12 hrs. on a hot plate of a temperature of 40° -200° C.........	·164″	20·3°/o	+1·7°/o	1112·6	52,733	344	3

Average decrease in B. strain after immersion in Acid = 297 lbs. per sq. in.
„ „ ultimate elongation „ „ = 0·8°/o

Table B.

Effect of Hydrochloric Acid on Unannealed Iron Wire.

Description.	Diameter, inches.	Ultimate Elongation.	Increase in ditto.	Breaking strain.	Breaking strain per sq. inch of section.	Increase in ditto.	No. of tests of wh. the mean is given
Unanneal'd iron wire immersed in acid 1 hour..	·136″	2·0°/o		lbs. 1193	lbs. 82.150		3
Same piece as above after being 12 hours in hot chamber of tem. 40°–200°C	·136″	2·83°/o	0·83°/o	1226·6	84,539	2,389	3

Dec. in B. strain after immersion in acid=2,389lbs. per sq. in. or abt. 3°/o
„ elongation „ „ =0·83°/o

Table C.

Effect of Hydrochloric Acid on Annealed Mild Steel (about ·22°/o Carbon).

Number of piece.	Diameter, inches.	Elongation after immersion in Hydcl. for 6 hours.	Elongation after being heated 12 hrs. to a tem. of 100 C.	B strain after immersion in Hycl. for 6 hrs.	B. strain after being heated 12 hours to a tem. of 100 C.
1	·135″	19°/o	16°/o	828 lbs.	840 lbs.
	·135″	19 „	17 „	824 „	836 „
	·135″	20 „	16 „	836 „	860 „
2	·135″	20 „	14 „	730 „	780 „
	·135″	20 „	13 „	730 „	754 „
	·135″	20 „	16 „	770 „	820 „
3	·135″	21 „	16 „	728 „	770 „
	·135″	20 „	17 „	742 „	800 „
	·135″	22 „	14 „	736 „	794 „
Average.	·135″	20·1 °/o	15·4 °/o	769·3 lbs. 53800lbs.pr.sq.in.	806 lbs. 56363lbs.pr.sq.in.

Decrease in B. strain after immersion = 2,563 lbs. or 4·56°/o
Increase in elongation „ = 4·7°/o

TABLE D.

COMPARATIVE EFFECT OF SULPHURIC AND HYDROCHLORIC ACIDS ON UNANNEALED CHARCOAL IRON WIRE.

Description.	Diameter inch.	Ultimate Elongation.	Decrease in ditto.	Breaking Strain.	Breaking Strain per sq. inch.	Decrease in ditto.	No. of tests of wh. result is a mean
				lbs.	lbs.	lbs.	
Charcoal Iron before immersion in acid...	·049″	2·3%	......	147·3	78,200	......	3
Ditto after 12 hours in hydrochloric acid and subsequent 5 hours in air at a temperature of 12°C.	·047″	1·17%	1·13%	122·7	71,930	6,270	3
Last piece after being on hot plate for 5 days at a temperature of 40—140°C...	·047″	3·5%	—1·2%	81·3	47,954	30,246	3
Charcoal Iron after 12 hours in sulphuric acid and subsequent 5 hours in air at a temperature of 12°C.	·045″	1·3%	1·0%	98·	61,800	16,400	3
Last piece after being on a hot plate for 5 days at a temperature of 40—140°C...	·045″	4·0%	—1·7%	104·6	65,995	12,205	3

All the above 15 tests were made from one coil.

Permanent decrease in B. strain after prolonged immersion in hydrochloric acid=30,246lbs. per sq. in.
„ „ „ sulphuric „ =12,205 „ „ „
„ increase in ultimate elongation after prolonged immersion in hydrochloric acid=1·2 per cent.
„ „ „ sulphuric „ =1·7 „

Table E.

Effect of Hydrochloric Acid on Unannealed Charcoal Iron Wire.

Description.	Diameter. inch.	Ultimate Elongation.	Decrease in ditto.	Breaking Strain.	Breaking Strain per sq. inch.	Decrease in ditto.	No. of tests of wh. result is a mean
				lbs.	lbs.	lbs.	
Charcoal iron before immersion in acid...	·046″	1%	......	134	80,240	......	6
Ditto after immersion in acid for 5 hours...	·045″	1·83%	......	110·6	68,616	......	6
Last piece after 7 days on hot plate...	·045″	3·00%	—2·0%	94·6	59,700	30,540	6
Charcoal iron after immersion in hydrochloric acid for 5½ hrs., then washed and dried in air 6 hours and again immersed 5hrs. in acid	·045″	1%	......	86·	53,320	......	6
Same piece as last heated for 7 days on hot plate	·045″	5·3%	—4·3%	90·6	56,172	34,068	6

All the above 30 tests were made from one coil.

Average permanent decrease in B. strain after prolonged immersion=32,304lbs. per sq. in.
„ „ increase in elongation „ =3·15 per cent.

Table F.

Effect of Hydrochloric Acid on Unannealed Mild Steel.

Description.	Diameter. inch.	Ultimate Elongation.	Decrease in ditto.	Breaking Strain.	Breaking Strain per sq. inch.	Decrease in ditto.	No. of tests of wh. result is a mean
				lbs.	lbs.	lbs.	
Mild steel before immersion in acid......	·0905″	1·66%	......	656·3	102024	......	6
Ditto after immersion in dilute acid 5 hrs..	·0893″	2·8%	—1·14	613·3	97979	+4045	6
Last piece heated in hot room 12 hours to a temperature of 40°—120°C.	·0893″	2·16%	—·5%	671·6	107295	—5271	6
Last piece heated 7 days in same hot chamber	·0893″	3·42%	—1·76%	701·	111992	+9968	6

Decrease of Breaking strain on immersion=4,045lbs. per sq. in.
Increase of Elongation „ „ =1·14 per cent.

Each result is the mean of 6 tests on different coils.

Extension of the Influence of Acid beyond the part immersed in the liquid.

Hitherto all the experiments have been made on iron totally immersed in acid solutions. It appeared to me, however, that very possibly the changes produced in iron and steel by immersion in acid might not be confined to the part in contact with the liquid only, but might also spread beyond and produce effects similar in quality but less in degree. With this view the following experiments were made.

Pieces of carefully selected charcoal iron, mild steel (about ·22 carbon), and hardened and tempered steel wire (about ·65 carbon), each about 35 centimetres long and of same thickness, were partly immersed in 4 tubes 13·5 centimetres deep, filled respectively with dilute sulphuric, hydrochloric, and nitric acids, and a saturated solution of common salt in water. After 72 hours the pieces of wire were pulled out and examined, when considerable differences became apparent.

1st. The pieces immersed in salt solution were freed from a very slight coating of rust on them before immersion, where they were in contact with the liquid, but above the surface of the liquid the rust remained as before. No decided alteration in toughness was apparent, and no bubbles were given off from the moistened fracture of the wire either of the part in the liquid or that out.

2nd. The pieces in nitric acid were slightly eaten away on the surface, the hardened steel least and the mild steel most, and the surface was covered with a fine black dust in the case of the charcoal and hardened steel. The moistened fracture did not bubble, however, in any of the pieces either in the part in or out of the acid, and no alteration in toughness was apparent from that before immersion, except in that part of the hardened steel covered with acid, where a possible though undecided diminution seemed to exist.

3rd. The surface action of hydrochloric acid was rather more marked than with nitric acid, and seemed to develope fibrous markings, while the nitric acid only fretted the surface.

The hardened steel was very little eaten away indeed, while the mild steel was half eaten away, the action being irregular and much more marked in some places than in others. The action on the charcoal was intermediate between the mild and hardened steel.

On moistening the fracture of the charcoal iron bubbles were given off in great abundance from the whole surface of the fracture. On trying the same experiment with the part adjacent, but not actually immersed in the acid, a few small bubbles were seen to rise from the surface of the fracture, even at a distance of 17c.m. from the surface of the liquid. These bubbles were found to increase in number and the toughness to decrease as you approached the surface of the acid. Moreover, when that part of the wire not immersed in the acid was broken, the bubbles arose almost exclusively from the centre of the fracture; while from the part immersed in the acid they arose from the whole surface, and took less time to attain their maximum.

No bubbles were visible to the unaided eye on the moistened fracture of the mild steel or on the hardened steel. That part of the mild steel immersed in the acid broke when bent like a pipe stem, 1 centimetre above the surface of the acid it was a little tougher, and at 2 centimetres distance it was as tough as before immersion, and would stand bending to and fro many times before breaking.

The hardened steel immersed in acid broke very short, but a little tougher than the mild steel; perhaps because it was not so much eaten away. At 1 centimetre above the surface of the acid it broke less short than in the acid, and the toughness seemed to increase with the distance from the acid until at 4 centimetres it broke as tough as before immersion.

4th. The effect of sulphuric acid was almost the same as that of hydrochloric, except that its surface action, particularly in the case of mild steel, was not so marked.

It thus appears that the fibrous nature of the charcoal iron allows the acid and absorbed hydrogen to pass up through the interior of the mass for some 17 c.m. above the surface of the acid, thus decreasing its toughness. The closeness of the grain of the steel and absence of fibre appear on the other hand to prevent this action. This difference in the behaviour of steel and iron is the more remarkable as the decrease of toughness consequent on immersion in acid is much greater with steel than with iron.

Further, it is well worthy of notice that nitric acid, which does not evolve hydrogen on contact with iron, does not appear to perceptibly diminish the toughness of iron or steel immersed in it, nor do bubbles arise from the moistened fracture of these metals after immersion in it, as they do with acids which give off hydrogen by their action on iron.

In view of the constantly increasing consumption of mild steel, and the proposal to introduce it in place of iron for ship building, the greater corrosive action of acids on mild steel than on iron has considerable interest. For it would seem to indicate that mild steel will more rapidly corrode than iron, and possibly this may render its use in shipbuilding to be attended with greater risk.

Mr. R. D. DARBISHIRE, F.G.S., gave an interesting account of Dr. Schliemann's Excavations and Discoveries on the Site of Troy, and exhibited some selected Photographs from his collections.

"Results of certain Magnetic Observations made at Manchester during the year 1873," by Professor BALFOUR STEWART, LL.D., F.R.S.

These observations were made in a small wooden house,

the property of The Owens College, erected in the garden of Professor Stewart, no iron whatever being used in its erection.

The latitude of the house is 53° 26′ N., and its longitude 2° 13′ W. The observations were chiefly made by Mr. F. Kingdon, assistant at the Physical Laboratory of Owens College, in the presence of students who were receiving instruction.

These conditions are not so favourable to minute accuracy as where the observations made are not combined with instruction to pupils: nevertheless it is believed that the following table will exhibit fairly accurate values of the magnetic dip and horizontal force for the year 1873.

The instruments used had been previously verified at the Kew Observatory. They consist of a Dip circle by Dover and a Unifilar by Elliott Bros. The dips are generally the mean results from two needles.

1873.	Dip.	Horizontal Magnetic intensity.
January	69° 19′·5	
February	69° 14′·6	
March	69° 17′·6	3·693
April	69° 18′·9	
May	69° 17′·7	3·686
June	69° 16′·1	3·684
July	69° 14′·6	3·704
August	69° 20′·2	3·704
September......	69° 23′·8	3·715
October.........	69° 16′·8	3·661
November......	69° 14′·4	
December......	69° 18′·3	3·686

PHYSICAL AND MATHEMATICAL SECTION.

March 3rd, 1874.

ALFRED BROTHERS, F.R.A.S., President of the Section, in the Chair.

"Results of Rain-Gauge Observations made at Eccles, near Manchester, during the year 1873," by THOMAS MACKERETH, F.R.A.S., F.M.S.

The rainfall of 1873 was much below the average fall, and very different in result from that of the previous year. Whilst the rainfall of 1872 was about 36·7 per cent above the average fall, the fall of the past year was about 12 per cent below the average. It is remarkable how little this deficiency of rainfall was noticed by persons who had no means of ascertaining the fact. Frequently I was asked during the year if the rainfall was not greater on the whole year than usual. This doubtless arose from the fact that the summer months, the only enjoyable season of the year, had a considerable excess of rainfall. The excessive fall in August had a bad effect upon the potato crop in the neighbourhood of Eccles. The least amount of rain fell in the spring and autumn months, though the greatest deficiency was in December. The number of days on which rain fell during the year was in excess of the average; but this excess was between April and September; the number of days of rainfall in the remaining months was below the average.

The following table shows the results obtained from a rain-gauge with a 10in. round receiver placed 3ft. above the ground.

Quarterly Periods		1873.	Fall in inches	Average of 13 years.	Difference.	Quarterly Periods	
Average of 13 years	1873.					Average of 13 years.	1873.
Days	Days						
51	45	January......	3·808	2·779	+1·029	7·481	7·047
		February ...	0·551	2·250	—1·699		
		March	2·688	2·452	+0·236		
46	52	April.........	0·699	2·078	—1·379	6·911	5·677
		May	2·052	2·085	—0·033		
		June	2·926	2·748	+0·178		
52	67	July	4·324	3·122	+1·202	10·296	10·783
		August	4·148	3·089	+1·059		
		September...	2·311	4·085	—1·774		
57	55	October......	4·587	4·271	+0·316	10·387	7·620
		November...	2·265	3·128	—0·863		
		December...	0·768	2·988	—2·220		
206	219		31·127	35·075	—3·948		

In the next table is given the results obtained from rain-gauges of two different kinds placed in close proximity in the same plane and 3 feet from the ground. The one has a 10in. round receiver and the other a 5in. square receiver. The large receiver had an excess over the small one in most of the months of the year, the exceptions being January, July, August, and October. The total difference of the fall in the two gauges was very small, being even less than the difference in 1872. The total difference in 1872 was over four tenths of an inch, but in 1873 it was barely over three tenths of an inch. And an average fall in both gauges over a period of six years shows a difference of only about the same amount. Thus the two gauges, though of different characters as regards their receivers, are good checks upon each other.

1873.	Rainfall in inches in 10in. round receiver, 3ft. from ground.	Rainfall in inches in 5in. square receiver, 3ft. from ground.	Differences	From 1868 to 1873. Average of 6 years' rainfall in inches in 10in. round receiver, 3ft. from ground.	From 1868 to 1873. Average of 6 years' rainfall in inches in 5in. square receiver, 3ft. from ground.	Differences
January	3·808	3·823	— ·015	2·987	2·976	+ ·011
February	0·551	0·543	+ ·008	2·250	2·209	+ ·041
March	2·688	2·660	+ ·028	2·380	2·347	+ ·033
April	0·699	0·641	+ ·058	2·192	2·162	+ ·030
May	2·052	2·000	+ ·052	1·906	1·871	+ ·035
June	2·926	2·880	+ ·046	2·600	2·557	+ ·043
July	4·324	4·359	— ·035	2·902	2·890	+ ·012
August	4·148	4·176	— ·028	2·857	2·797	+ ·060
September ...	2·311	2·188	+ ·133	3·931	3·868	+ ·063
October.........	4·587	4·630	— ·043	5·124	5·097	+ ·027
November ...	2·265	2·207	+ ·058	2·828	2·851	— ·023
December......	0·768	0·713	+ ·055	3·308	3·291	+ ·017
	31·127	30·820	+ ·307	35·265	34·916	+ ·349

In the next table I give the results obtained from two exactly similar gauges placed at different heights from the ground and free from every interference. Each gauge has a 5in. square receiver, and the one is placed 3 feet and the other 34 feet above the ground. The total fall in the one 3 feet from the ground was 30·820 inches, and in the one 34 feet from the ground it was 25·401 inches, for the year 1873. The difference between the fall in the two gauges is 5·419 inches, or about 17 per cent less rain fell in the higher than in the lower gauge. In the following table I give the average fall in the same gauges for 6 years, and by comparing the results it will be found that the rate per cent of the difference of the rainfall in the two gauges is the same as it is for last year.

1873.	Rainfall in inches in 5in. square receiver, 3ft. from the ground. 1873.	Rainfall in inches in 5in. square receiver, 34ft. from the ground. 1873.	From 1868 to 1873. Average fall of rain in inches for 6 yrs. in 5in. square receiver, 3ft. from the ground.	From 1868 to 1873. Average fall of rain in inches for 6 yrs. in 5in. square receiver, 34ft. from the ground.
January	3·823	2·799	2·976	2·130
February	0·543	0·349	2·209	1·656
March	2·660	2·142	2·347	1·846
April	0·641	0·570	2·162	1·858
May	2·000	1·750	1·871	1·583
June	2·880	2·633	2·557	2·288
July	4·359	3·793	2·890	2·566
August	4·176	3·378	2·797	2·378
September ...	2·188	1·894	3·868	3·322
October.........	4·630	3·804	5·097	4·228
November ...	2·207	1·764	2·851	2·177
December......	0·713	0·525	3·291	2·760
	30·820	25·401	34·916	28·792

I thought it might be interesting if the ratios were given of the excesses of rainfall measured at 3 feet from the ground over the amount measured at 34 feet above the ground. These ratios appear in the following table. It is curious to observe that the ratios of the total amounts that fell in these gauges, both for 1873 and for an average of 6 years, are identical, the ratio of the difference between the fall in the upper gauge and that in the lower being 0·824. And even in the heavy rainfall of 1872 it will be found that the ratio of the deficiency between the fall in the two gauges was nearly similar, being 0·855. But when the monthly ratios are examined, either for the year 1873 or for the average rainfall in the gauges for 6 years, it will be seen that the greatest difference of fall happens in January and the least in June. The oscillations, too, of the differences between the fall of 1873 and the average fall of 6 years is very marked. On the theory that the excess of rainfall in the lower gauge is due to the particles of invisible vapour in the air between it and the higher gauge coalescing with the falling rain drops, the table seems to show that in

the summer months, and particularly in June, there is relatively less of this vapour in the air below a height of 34 feet, and that there is relatively more of it in the winter months, and particularly in January. Hence the maximum of dry air on the ground is in June, and the minimum in January. This can only be due to a very rapid convection of warm air with its attendant vapour from the surface of the ground upward. A consideration of this law in its action upon public health cannot be too much regarded.

MONTHLY AND ANNUAL RATIOS OF THE EXCESS OF RAINFALL MEASURED AT 3 FEET FROM THE GROUND OVER THE AMOUNT MEASURED AT 34 FEET FROM THE GROUND.

	Ratios of such rainfall for 1873.	Ratios of such rainfall for an average of 6 years from 1868 to 1873.
January	·732	·715
February	·642	·749
March	·805	·786
April..................	·889	·859
May	·875	·846
June	·914	·894
July	·870	·887
August...............	·808	·850
September	·865	·858
October	·821	·829
November	·799	·763
December............	·736	·838
Annual ratios...	·813	·822

In the next table I give the fall of rain during the day from 8 a.m. to 8 p.m., and the fall during the night from 8 p.m. to 8 a.m. The amount of rain that fell during the day exceeded, as usual, the fall during the night, and this happened in every month of the year excepting January, February, and December. The total excess of the day over the night fall for last year was 2·846 inches. In 1871 the excess was 4·136 inches, and in 1872 it was 1·891 inches.

1873.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Differences between Night and Day fall.
January ...	1·113	2·710	+1·597
February ...	0·249	0·294	+0·045
March	1·579	1·081	—0·498
April.........	0·502	0·139	—0·363
May	1·165	0·835	—0·330
June	2·194	0·686	—1·508
July	2·565	1·794	—0·771
August	2·424	1·752	—0·672
September..	1·218	0·970	—0·248
October......	2·423	2·207	—0·216
November...	1·153	1·054	—0·099
December...	0·248	0·465	+0·217
	16·833	13·987	—2·846

In the next table I present the average day and night rainfall for 6 years. An examination of this table continues to confirm the experience of previous years, that the day fall exceeds the night fall so far as the amount of the whole year is concerned. But curiously enough this table of 6 years' average gives an excess of night fall in the same months that were shown in a table of 5 years' average which I read at the Section last year, those months being January, February, August, September, November, and December. It is however remarkable how close the total day and night rainfalls are to each other, for though the 6 years' average shows a difference of double that of the 5 years', yet it is only about 4 per cent on the total fall.

Average of Six Years from 1868 to 1873.

1873.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Differences between Night and Day fall.
January	1·321	1·655	+0·334
February	0·919	1·288	+0·369
March	1·375	0·970	—0·405
April	1·279	0·883	—0·396
May	1·206	0·666	—0·540
June	1·447	1·110	—0·337
July	1·713	1·177	—0·536
August	1·350	1·447	+0·097
September.....	1·773	2·094	+0·321
October	2·634	2·463	—0·171
November......	1·367	1·484	+0·117
December	1·448	1·843	+0·395
	17·832	17·080	—0·752

Ordinary Meeting, March 24th, 1874.

Rev. WILLIAM GASKELL, M.A., Vice-President, in the Chair.

"On some of the Perplexities which the Art and Architecture of the Present are preparing for the Historians and Antiquarians of the Future," by the Rev. BROOKE HERFORD.

One of the most interesting elements in historical and antiquarian studies is the consideration of the age of the various remains which have come down to us from the past. It continually occurs that points of great importance depend upon our being able to assign an approximate date to a document, an inscription, or some architectural feature of a building. It is thus that vague tradition is often eked out by corroborative fact, and questionable chronicles become susceptible of verification. Even in cases in which it is out of the question to assign actual dates, it is something to be able to be sure of the general period to which an object belongs; and even the *iron, bronze,* and *stone* periods afford distant landmarks by which we may grope our way back into an antiquity which may not always be so dim as it is at present.

Now all who have gone into the study of the dates or periods of ancient remains or monuments, must have been struck by one feature which is almost universally characteristic of them. I refer to the marvellous reliability of whatever indications they carry in themselves of style or stage of development. Once get back beyond a certain borderland of confusion and deception, and almost everything speaks for itself. You can tell—approximately, of course—*when* it was done, by the *how* it is done. The fact is that as the arts of life gradually developed men did

everything simply according to the best idea they had attained. They worked by the clearest light of their own time. There was a sort of sturdy self-respect in their workmanship. They did not think that their own best forms were prosy and commonplace, and when they wanted something unusually beautiful run after the ruder forms of an earlier age. They had the thought of the beautiful, but they seem to have been happily innocent of that sentimental distortion of the sense of beauty which leads to the artificial manufacture of the picturesque. So you tread confidently and firmly as you trace back the ways of the ancient life. You may be walking among rude and imperfect things, but at any rate you are not walking among shams. The student of old manuscripts will tell you the age of a Greek codex by the uncial or cursive character in which it is written. If you find a tomb inscribed in the old, so-called "Lombardic" characters, you feel pretty sure that it is not later than the 13th or beginning of the 14th century, nor can it be much earlier, for previously the tombstones, coffin lids, had crosses, but not inscriptions on them. When you come across a rude circle of stones upon the moors above Ilkley, you may doubt if it was a hut circle, or a Druid Temple; but at any rate you are not haunted by a misgiving that it was only a freak of some early monk of Bolton Priory, with a taste for the picturesque. If you are pointed to an apparent date of 1174, like that which stands on the gable of an old house near Sowerby Bridge, you know, *at any rate*, that it cannot really be of 1174, because the Roman numerals were still universally in use, and the so-called Arabic figures hardly even known in England, till two centuries later. If you meet with an early undated specimen of printing and find some words in it in italics, you know that it cannot be earlier than 1501 when the great Venetian printer Aldus Manuzio first introduced the new slanting letters which he had had cut, in the series of works which have

given the Aldine classics an imperishable name. Or, again, if you meet with an old parchment engrossed in the curious black-letter which is known as balloon writing, you can form a pretty fair conjecture that it dates back to the time of the Edwards.

But it is in Architecture that we come upon the most striking and important illustrations of the way in which each age, going a little beyond the age before, believed in its own improvements, and did simply the noblest thing it knew. I hesitate to speak of an art of which I know so little technically, but even the knowledge that an ordinary lover of old buildings attains suffices to illustrate my point, and also to show its value. You can trace our architecture, from the great Norman builders, who first taught the English how really to *build*, down to the exaggeration of the 16th and the debasement of the 17th centuries, by unbroken steps,—Norman, Transitional, Lancet, Geometrical, Curvilinear, Perpendicular,—every one of which opens up new studies of interest.

And even later still, though the architecture of Elizabeth's time is only reckoned of the poorest (and in ecclesiastical architecture there is nothing, for the church was too much weakened by the Reformation to do much building), yet in domestic architecture we have a type of house with many and sometimes fantastic gables, and massive mullioned and transomed windows, which at any rate *stands for itself*, and is an interesting study in many of our halls and manor houses.

Of course, all these cannot be divided by sharp and unerring lines of date. They cannot be divided, precisely because they are the outcome of true growth, and growth implies continuous and indivisible processes. But the order of growth is wonderfully observed. Sometimes even its minuter steps can be apportioned to their special dates with a curious exactness. I remember hearing Mr. Edmund

Sharpe, an architect and antiquarian who has studied the old Cistercian abbeys as no other living man has done, pointing out at Furness Abbey one special moulding—the plainest possible inverted volute—as really dating the capitals on which it appears to within about 20 years. It is seldom, of course, that any such exactness can be attained. The rate of progress was not equal everywhere, and in remote country places the old forms lingered after the great monastic builders had left them behind.

As to the practical value of all this in the study of history, one or two illustrations will suffice. Often it happens that much of the history of a church or an abbey is contained in the silent testimony of the successive stages of its architecture, or in the remains of older work embedded in more recent masonry.

For instance: you visit some little country church. Its general character is that of Henry VII.'s time—you know it by the Perpendicular windows and the spandrelled doorways. But look a little closer. In the chancel perhaps are a couple of simple Lancet windows, and above them are two or three quaint corbels, which never originally supported that late battlemented parapet; and underneath the comparatively recent porch is an old semicircular arch, and if you look carefully about the walls you will probably find here and there, built in, fragments of carved stone—perhaps pieces of old stone-coffin slabs with crosses visible upon them—which tell of a much earlier church; and sometimes you find fragments of old interlacing scroll work, such as that upon the Runic crosses of Ilkley, which carry back the interest still further, and tell of Saxon worshippers.

And now if you have gone with me in this appreciation of the self-respect which in old times men shewed for the best art of their own day, and of the value of the trustworthy indications of date, arising out of this orderly development, which everywhere present themselves, it will hardly

need that I should enter very much into detail as to the perplexities which the art and architecture of the present are preparing for the students of future times. For every step in which I have been shewing these trustworthy characteristics of the past must have reminded you how different things are to-day. I tremble sometimes to think of the curses which may some day be heaped upon this self-complacent nineteenth century, with its great affectation of taste and art, by those who in some remote future may have the task of disinterring, and endeavouring to interpret our monuments! They will find inscriptions in every variety of character, Lombardic, old English, the antique Roman type of the end of the 17th century, and—but very sparingly—in the beautiful characters which our modern type founders have elaborated,—such type as never was in the world before, but of which modern Englishmen seem ashamed. They will find drinking fountains of Queen Victoria's reign inscribed in characters which would betoken an origin under the Plantagenets; and ridged tombstones of decent Manchester merchants hardly distinguishable from those of the old, spurred and belted border-knights who compounded for their sins by leaving estates to the monks on condition of burial within the cloisters. They will find books printed in carefully imitated types of the sixteenth century referring to matters which ordinary history would have led them to believe happened in the nineteenth.

But it is in matters of architecture that they will experience the most bewildering perplexity. Imagine, if it is possible, Manchester disinterred from the superincumbent mould of three thousand years by some enterprizing relative of Mr. Macaulay's celebrated New Zealander, who has read of "the Manchester School," and desires, like another Dr. Schlieman anent Troy, to prove to his incredulous countrymen, that Manchester was a real place, and its school a veritable seat of learning! Imagine the curious wonder

with which the Cyclopean architecture of the shops underneath the Exchange would be compared with the fragments of the friezes and mouldings which might be found among the ruins. Imagine the perplexity with which the portico of the Infirmary would be compared with the facades of some of the Portland street warehouses, and all attempted by some systematic thinker to be co-ordinated with the Assize Courts and the New Town Hall. As for the endeavour to ascertain anything about our domestic architecture, that would be hopeless indeed. But there is one consoling thought. It is possible that with regard to this at any rate the perplexity may never occur; for it is few indeed of our modern houses that will stand into a second or third century!

Perhaps the most important feature in this matter is that what we are doing by this artistic and architectural confusion will not stop with the perplexity which it will cause concerning the remains of our own time. That might be suffered. Some may say, our buildings, at least our considerable public buildings, are all *dated*—so are our books, and our sculptured inscriptions. True. But the difference between those of our day *which are dated*, and those of six centuries ago, which are *not* dated, will grow less distinguishable every century, until at last they will all be touched with one equal aspect of antiquity, and the utter confusion which will then appear among those which *are* dated, can hardly fail to bring equal confusion, and at last discredit upon those which *are not*.

I do not think that this is a light matter, though I have pointed out in passing a certain ludicrous aspect which it undoubtedly possesses. I think it points to deeper defects in the mental and moral life of the age: to a craving for the excitement of the picturesque rather than an appreciation of the really beautiful; to a want of originality in the higher forms of art, that is poorly compensated by the skill with which we can lay Etruscan and Pompeian vases or

mediæval cathedrals under contribution; and to a miserable depreciation of our own contemporary forms, even when, as in the case of our printed characters, they are really of surpassing clearness and excellence.

"A Few Observations on Coal," by E. W. BINNEY, V.P., F.R.S., F.G.S.

Of late years much has been written on the structure of coal and the various vegetable remains of which it is composed. Observers examining different coals under the microscope have, as might be expected, come to different conclusions. Some of them have found little else than the remains of spores and spore cases, others only scalariform and cellular tissues and a few spores, and a third class little trace of any structure whatever in the specimens they examined. Splint or hard coal would generally afford the results first named, soft caking or cherry coals the second, and cannel coals the third.

Soft coals yielding a large amount of charcoal enclosed in bright coal nearly always show plenty of structure in the "mother coal," as well as a few macrospores in the bright portions. Macrospores are nearly always found in abundance in splint and hard coals. In cannel coals they are sometimes found as well as the cellular and scalariform portions of plants.

For many years macrospores were known by the names of spore cases and spores. They could be easily observed by the naked eye in the black parts of the coal, and they were generally considered as sporangia, but Professor Adolphe Brongniart in 1868 described a cone (*Lepidostrobus Dabadianus*) having sporangia full of very minute spores, in fact microspores, in the upper portion, and sporangia full of macrospores, which had been so long known, in the lower part. These observations of Brongniart have been amply confirmed by specimens from the British coal fields.

Thirty years ago it was considered that the soft or cherry coals were chiefly composed of the remains of large plants such as *Sigillaria, Lepidodendron,* &c., while caking coals were formed of plants of a lesser size and much bark, for it had then been observed and since confirmed, that the outsides of *Sigillaria* and other fossil trees, sometimes reaching to two or three inches in thickness, were chiefly composed of bright soft coal showing little traces of structure.

In the great lawsuit which was tried at Edinburgh more than twenty years since as to the nature of Boghead coal, much evidence was given as to its structure, some witnesses finding in it scarcely anything else than the remains of vegetables, whilst others found only a stray portion of scalariform structure or a macrospore. Both Boghead and the other brown cannels of Scotland are now generally considered to afford but little evidence of vegetable tissues under the microscope, although numerous remains of *Sigillaria* and other common coal plants cau be seen by the naked eye in them. Notwitstanding that they are so rich in volatile matter, and far exceeding other coals in their yield of paraffine and paraffine oils, they contain from 25 to 30 per cent of mineral matter in the form of ash. This circumstance has induced certain scientific men to class them as shales rather than coals, notwithstanding that they have the specific gravity of coals.

Some years since, when describing several fossil cones affording both kinds of spores, he expressed an opinion that the yellow matter seen in the vesicles of Boghead coal was nothing but microspores composed of paraffine or a similar hydrocarbon, and that they were driven off by heat in the form of a yellow vapour, leaving nothing behind but a spongy mass of earthy and carbonaceous matter. The evidence that caused him to come to this conclusion was that the microspores contained in the upper sporangia of *Lepidostrobus Harcourtii* had all the appearance of crude

paraffine, and were of the same yellowish brown colour as powdered Boghead coal, and that the latter substance was composed nearly altogether of microspores and mineral matter. So far as his observations extended, microspores had not been observed in coal, although plenty of macrospores, which are generally $\frac{1}{20}$th to $\frac{1}{16}$th of an inch in diameter, and easily seen by the naked eye, had long been noticed.

He had some time since directed his friend Mr. J. W. Kirkby, of the Pirnie Coal Company, to examine the Fifeshire seams of coal in search of microspores, most of those beds yielding macrospores in great abundance, and that gentleman had lately furnished him with the specimens now exhibited, both splint and soft coals, but especially the former, affording the two kinds of spores. On burning the yellow coal composed of microspores, a most brilliant flame and a peculiar empyreumatic odour like that from burning Boghead coal were produced, whilst the splint coal full of macrospores only burnt and smelt like ordinary hard coal, thus clearly showing that these two kinds of spores differed very much in their inflammable properties and odours given off, and that such properties were certainly not due to the larger spores but most probably to the smaller ones.

The compressed lenticular bodies in the splint coal were formerly of oval and spherical forms with a triradiate ridge on one half, and although their exterior was composed of a brown coriaceous substance, their interior was full of white carbonate of lime or bisulphide of iron according to the nature of the matrix in which they were found; thus suggesting the idea of their having been filled with granules of starch when in a living state, which it is probable they would have been in case of their being germinating spores.

He and his late partners at Bathgate, when manufacturing paraffine oil there, had tried various means, by subjecting Boghead coal to the action of ethers and naphthas, to dissolve out paraffine from it, but they had never succeeded,

so the yellow matter in the coal may probably be changed into paraffine by the heat employed in distillation. Dr. Schorleimer, F.R.S., who has been so kind as to examine the microspores found in the Muiredge splint coal, is of opinion that they are not composed of paraffine, but some other hydrocarbon. This may, therefore, change into paraffine by the application of heat in a similar way to what the yellow matter in Boghead coal does.

The macrospores are about 320 times the size of the microspores and constituted the germinating spores, while the microspores were the fertilizing agents, both having been contained in one cone.

Several specimens of fine bright soft coal, between 2 and 3 inches in thickness, taken from the outsides of *Sigillaria* and other fossil trees, were exhibited. In these there was no appearance of charcoal, spores, or vegetable structure, and in every respect they resembled the black shining parts of soft cherry and caking coals, which generally afford no distinct traces of vegetable structure. Hence from his observations he was led to conclude that soft or cherry coal was chiefly composed of the bark, cellular tissue, and vascular cylinders of coal plants with some macrospores and microspores.

That caking coal had much the same composition, except that it contained a greater proportion of bark in it.

That splint coal had a nearly similar composition, but with a great excess of macrospores.

That cannel coal, especially that yielding a brown streak, was formed of the remains of different portions of plants with a great excess of microspores, which had long been macerated in water.

These conclusions were arrived at merely as to the composition of the different kinds of coal. No doubt each seam would be materially affected by the nature of the roof, whether the latter was an open sandstone or a close and air-

tight black shale or blue bind, for the former would allow the free escape of gaseous matter, and the latter would prevent its escape. It is well known that the character of the roof has a deal to do with the quality of the coal under it.

Ordinary Meeting, April 7th, 1874.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

Mr. J. Sidebotham and Mr. J. A. Bennion were appointed Auditors of the Treasurer's Accounts.

The CHAIRMAN exhibited to the meeting some portion of the cast iron roof from the Salford Station of the Lancashire and Yorkshire Railway, which after having been up for a period of four years was so much corroded and damaged that it had to be taken down. He attributed the effects to sulphuric acid and soot arising from the combustion of the coal used in the locomotives passing under it, aided by the action of steam and vibration. He referred to a paper by himself communicated to the Society and published Vol. II. (2nd series) of its Memoirs, on the effects of old coal pit water on cast iron, where similar results had been produced by sulphuric acid, carbonaceous matter, and water; also to a case alluded to by one of the most distinguished members of the Society, the late Dr. W. Henry, F.R.S., of the rotting of cast iron by the escape of steam from the junction of a pipe embedded in charcoal. Of course the rate of decomposition much depended on the quality of the iron; but as that metal was now so much employed in building and mining operations, he considered it desirable to bring before the public every instance that came to his knowledge where it had been damaged or decomposed.

"On the Action of nascent Hydrogen on Iron," by WILLIAM H. JOHNSON, B.Sc.

In a paper read before the Society last year, I showed that a piece of iron immersed in hydrochloric, sulphuric, or other acid which evolves hydrogen by its action on the metal, on breaking gives off bubbles of gas from the surface of the fracture. It subsequently occurred to me that these bubbles might be produced by subjecting the metal to the action of nascent hydrogen for some time, and without the aid of acid at all.

To test this I connected two pieces of iron wire ·07″ diam. respectively with the copper and zinc plates of a battery of 50 Daniell's cells and immersed them in a vessel of Manchester town's water at a distance of one inch apart. On closing the current, bubbles of hydrogen were given off from the wire connected with the zinc, but none from the wire connected with the copper, the oxygen liberated at the pole apparently forming oxide of iron which in 12 hours formed a thick smudge at the bottom of the vessel. After 24 hours the surface of the wire connected with the zinc was unchanged, but on moistening the fracture bubbles were given off abundantly just as if it had been immersed in acid. The other wire, on the contrary, though much oxidised and eaten away, did not give off bubbles when broken.

A variety of experiments were made in the same way with pieces of wire varying from 3 to 20 inches long and immersed from 5 to 24 hours, ½ inch to 4 inches apart in pure Manchester town's water, all with similar results. With this exception, that when the wire connected with the zinc was of steel, no bubbles were visible to the naked eye, just like steel after immersion in acid. Twenty-four hours in a warm room restored the iron to its original state, and no bubbles were seen on fracturing it.

The water in the last experiments was then replaced by

an aqueous solution of caustic soda, when after two hours the moistened fracture of the wire connected with the zinc pole of the battery was found to bubble. Twenty two hours' longer immersion, the battery working all the time, caused the bubbles to be more abundant; the toughness of the wire was also diminished, and its surface was blackened. The wire at the positive pole was however unchanged either on the surface or in toughness.

Three pieces of wire, each 12 inches long, were immersed in hydrochloric acid about 1·2 sp. gr., one being connected with the positive pole, the other with the negative pole of the battery, and the third unconnected with the battery. At the expiration of half an hour the two last pieces were found to bubble on being fractured, and were also more brittle. The one however connected with the positive pole was not in the least affected. Thus showing that simple immersion in acid is not sufficient to produce in iron the remarkable changes before described, unless it be accompanied by evolution of hydrogen.

In conclusion, if the oxidation of the surface of iron be as a rule accompanied by the absorption of nascent hydrogen into the interior of the iron, then the diminution of strength and toughness consequent on this will affect iron ships, telegraph cables, and other structures in which iron is largely used and which are constantly immersed in water.

"Does the Earth receive any Heat directly from the Sun," by HENRY H. HOWORTH, Esq.

The term heat is one of unusual ambiguity. It has at least two meanings, which, if they do not exclude one another, are at least not commensurable. Their indefinite and careless use has created great confusion. The first meaning connotes the feeling heat, a purely subjective matter, whose investigation is a proper subject for metaphysical students, but with which we have nothing to do

at present. The second meaning connotes objective heat, the force heat, that phenomenon of matter whose effects we can measure in certain definite ways. It is with this second meaning that we are concerned to-night. Having limited our subject somewhat, I wish to limit it further. The science of heat concerns itself with two main subjects, first, the transcendental problems which deal with the nature of heat, which endeavour to explain it as a form of motion, &c., &c. These conclusions may or may not be eventually confirmed by experience. At present they are powerful and ingenious theories which enables us to map out our knowledge to arrange and classify it, but I humbly crave permission to doubt whether any of them may be considered a final solution and *pro tanto,* I must have my hands free. This is not really of consequence in our discussion to-night, for I have no intention of entering into such a difficult and crooked controversy. The nature of heat is, in fact, outside our present subject. Eschewing the transcendental problems we shall very briefly consider only the direct effects of heat upon matter. So far then as our experience can take us heat is a peculiar condition or phenomenon of matter, which we can modify, increase, or lessen, which is universally present in greater or lesser intensity, and which we measure relatively by a comparison with a certain fixed standard. However produced, whether by chemical action, by percussion, by friction, &c., &c., there is one result which seems universal and which may be considered to be the correlative of heat. This result is universally present and modified in various ways it is probably the only result. So that if we exclude the feeling of heat we may define heat by using this result as an alternative term. The addition and abstraction of heat are correlative respectively with an increase and decrease in the bulk of the matter operated upon. If we add heat we increase the bulk of the substance ope-

rated upon; if we abstract heat we cause the portion of matter to contract and shrink, and the bulk is increased and diminished in certain definite proportions; and not only so, but the opposite holds good also, namely, that if we cause a portion of matter to shrink we must in doing so abstract heat from it, while if we increase its bulk in any way without adding more matter we must, willingly or unwillingly, add a corresponding portion of heat. I believe this is accepted as an axiom in physics by the best judges and accepted as universally true. Formerly some substances, such as bismuth, water between certain temperatures, &c., were quoted as exceptions to this rule, inasmuch as they expand in solidifying; but they are only apparent exceptions, for their aberrant behaviour has been shown, as I think beyond doubt, to be a phenomenon of crystallisation. Speaking of the most notorious case, that of water, Tyndal says: "The arrangement of the atoms of water when solid require more room than they need in the neighbouring liquid state. No doubt this is due to crystalline arrangement. The attracting poles of the molecules are so situated that when the crystallising comes into play, the molecules unite so as to leave larger interatomic spaces in the mass; we may suppose them to attach themselves by their corners, and in turning corner to corner to cause a recession of the atomic centres. At all events their centres retire from one another when solidification sets in. By cooling then this power of retreat and of consequent enlargement of volume is conferred."—Tyndall's *Heat as a Mode of Motion*, 107. These exceptions then I hold to be no exceptions at all. There is one substance, namely indiarubber, whose behaviour is eccentric and not explainable in this way, nor at present, so far as I know, explainable at all; but with this solitary exception, whose *raison d'être* I have no doubt will be shown to be as consistent with the general law as those of substances such as water, bismuth, &c., we may safely conclude from our experi-

ence of matter that it is a universal law that heat and expansion, cold and contraction are correlative terms.

Having laid down an abstract axiom, let us now apply it to a concrete example, namely, the earth.

The old notions about the stability of the earth when compared with the mobility of the sea have been long since exploded. We now know that there is no such thing as absolute terra firma. We know that the earth in a less degree is mobile like the water, rising here and sinking there in apparently restless pulsations, waves, swellings, and subsidings. This being so, it becomes an interesting problem to determine whether these risings and sinkings compensate one another; that is, whether a rising in one neighbourhood corresponds to a sinking elsewhere, or whether the whole periphery of the earth is undergoing enlargement or diminution, is stretching or shrinking. To decide this by direct experiment is not easy, for since water is our gauge the same relative effect will be produced either by the sinking of an ocean-bed or the rising of an adjacent continent. But we have a considerable amount of evidence notwithstanding which points, so far as I know, in one direction and one only. There is first the a priori evidence.

There are two sciences which deal with the inner constitution of the earth: astronomy, which deals with it as a part of the great macrocosm, the universe; and geology, which deals with its inner life as a universe of its own. The question of the alteration in the earth's bulk has naturally been discussed by both astronomers and geologists, and discussed too from very different points of view, but both are agreed in one conclusion, namely, that the earth is shrinking.

Since the days of La Place the Nebular hypothesis has been generally received by astronomers as the one which best meets observed facts. This hypothesis predicates the existence of gravitation everywhere, and shows how by its influence the various heavenly bodies have become con-

densed from nebular matter. It predicates that this force is still active everywhere, and that everywhere within our observation we have a condensation of matter in progress, matter condensing from a highly diffused condition to one of greater density. Thus each member of our system, it is argued, is gradually and surely nearing the sun and at the same time is shrinking, and the various planets are in fact in so many stages of evolution, and exhibit for us the various phases which the earth has passed through and will pass through before it is landed in the sun. And the most ingenious and successful of our analytical astronomers and physicists combined, Sir William Thomson, has compared our universe to an elaborately constructed clock which is inevitably and surely running itself down, until it has exhausted its various forces and until each component member has fallen into the common centre. This is elementary enough. I only quote it to show that the evidence of astronomy is, that the earth is contracting, and that its periphery is diminishing in area.

The conclusion of geology for our purpose is the same. It is argued by geologists that the earth was originally in an incandescent state, and that it has assumed its present shape after a gradual cooling, that is, a gradual contraction, which is still in progress. These are the words of Mr. Geikie, one of the most recent and competent authorities: "Among the geologists of the present day there is a growing conviction that upheaval and subsidence are concomitant phenomena, and that viewed broadly they both arise from the effects of the secular cooling and consequent contraction of the mass of the earth." On both grounds therefore, namely, those upon which astronomical and geological arguments are based, is it established that the earth is shrinking.

As the fire syringe discharges a certain amount of heat when sudden pressure is applied, which heat we can collect

and measure, so we ought to be able to measure the amount of heat produced by the contracting of the earth's crust, although we cannot measure that contraction with a plummet. As we go down through the crust we ought to meet with evidence that the pressure of the strata has increased the temperature, and this we in fact do. In going down towards the centre of the earth we find an increase of temperature so wonderfully constant in all latitudes that we are constrained, if we accept the nebular hypothesis, to argue that this results, as it ought theoretically to result, from the pressure of the strata, *i. e.*, from the force of gravity. In some letters that I have recently written to "Nature," I have tried to show how the areas of upheaval and subsidence on the earth are distributed, my conclusion being that the areas of depression are distributed about the Equator, while the areas of upheaval have their foci at the Poles, that the earth is being strictured about the Equator, and thrust out in the direction of its shortest axis; in other words, is shrinking in the region where the temperature is the highest, the tropics, and is stretching out in the regions of cold, or the Polar regions.

Now let us see how far we have travelled. We have postulated that all shrinking matter is giving out heat. We have shown that the *a priori* evidence is conclusive that the earth is a shrinking mass. We have also adduced evidence which shows that the earth is in fact hot where it should be hot, and cold where it should be cold, in accordance with the law of contraction; and it seems to me to follow necessarily that the earth is giving out heat,—is in fact a furnace, a heat producing substance. This seems to be inevitable.

I need not stay to argue that both popularly and also among scientific men it is held as a cardinal doctrine that the earth receives a large quantity of its heat directly from the sun, and elaborate calculations have been made to show

the terrific quantity of heat so received and the terrific energy which this represents. We are taught that of the sun's heat which beats on the earth one portion is reflected, another is absorbed, and that the latter is what we can alone recognize by our instruments, and whose energy is the subject of calculation. But the absorption of heat means, as we have argued, an increase of bulk, therefore whatever heat the earth absorbs must go to increase the earth's size. It is only on this condition that the earth can absorb heat at all, and it is only by the increase in bulk that we can in fact measure or gauge the heat.

But if the earth be shrinking it is clear that it must give out more heat than it absorbs, it must in fact produce enough heat to neutralize the expansion caused by the sun and some besides. The excess being measured by the amount of contraction that the earth is undergoing. But this means that any heat it receives from the sun is more than neutralized by its own heat, so that if the sun gave us no heat at all and the earth continued to contract, as it does now, it would not be affected in temperature, save perhaps in becoming even hotter, for if we receive heat from the sun which, when absorbed makes the earth expand, it is clear that a portion of the contracting force of the earth is spent and exhausted in neutralizing this expansion, which would be set free in the form of heat if this had not to be neutralized. But I confess that having brought the argument to this point I am constrained to go a step further, and to say that if the earth be independent of the sun for its heat, that if independently of the sun altogether it is throwing out an amount of heat equivalent to the amount of its contraction, that it is unnecessary and unphilosophical to postulate the sun as a source of heat, and that we are bound, paradoxical as it may seem, to conclude that the earth does not receive any heat directly from the sun. This conclusion seems inevitable, and if it be, we must face the

various difficulties that suggest themselves at every turn and find a solution to them consistent with it.

I believe these difficulties are not hard to meet. It will be noticed that in the question I propounded at the head of this paper I use the word directly or immediately. When I say that the earth is itself the furnace which supplies the phenomena of heat which we study and feel I do not mean that it does so entirely *per se*, and without any assistance from without. I predicate all through that such heat as the earth possesses is induced in it by the force which is contracting it, by the gravitating force, and this force, so far as our evidence goes, is derived nearly altogether from the sun. My argument is that the heat of the earth is mediately, but not immediately due to the sun's influence, that the sun's influence causes the earth to contract, and that in contracting heat is squeezed out of it. That we derive no heat whatever *qua heat* from the sun, or to use a simile, the voltaic battery supplies the electric current which induces the magnetism in the iron but does not itself furnish the magnetism.

This at once removes a vast quantity of apparent difficulty, for wherever the sun beats there it exercises its influence of gravity, and the result is invariably an induction of heat, so that winter and summer, day and night, are dependent for their varying temperature on the sun, although in a different manner to that popularly supposed.

There is another and a more palpable difficulty that obtrudes itself at once upon one's notice in defending the position I am arguing for. It is said: surely, if we step out of the sunshine into the shade, or *vice versa*, and exclude the radiated heat from the ground we shall find direct evidence both from our feeling and from the usual effects of heat that the sun's rays are distinctly hot, that in beating on our hand, on a moist surface, &c. &c., symptoms of absorption of heat at once present themselves—the hand grows

hot, the moisture evaporates, &c. And this is true in a larger way of the superficial layer of the ground, which feels the intermittent effects of day and night, summer and winter, by absorbing more or less heat. How then do we account for this, for the heat must be absorbed from somewhere, and the only practicable source is the direct sunshine. My answer to this is very short. If we ascend through the atmosphere we rapidly find the temperature becomes lower, that as we get nearer the verge of the atmosphere, and therefore nearer the point where there is no atmospheric absorption of the solar rays we find the temperature to lower so rapidly that we are justified in concluding that if we could get outside the atmosphere altogether we should find the scorching of the hand, the absorption of moisture, and the various effects we attribute to heat reduced to a minimum, and are justified in concluding that if the envelope of the atmosphere were removed, if the earth were to be, such as the moon is, without an atmosphere, that its surface temperature would be but slightly affected by the direct sunshine. The effects of the sun's attraction would be distributed throughout the mass of the earth, causing a general contraction and a general relative temperature with its focus at the centre, but there would be no surface layer of an aberrant and peculiar temperature, and few or none of the effects we now find in the direct sunshine. I argue, and I think I am justified in arguing, that these peculiar effects are due entirely to our having an atmosphere, to the fact of there being a medium between the sun and ourselves. If this be so, it is clearly most consistent with our contention, for the sun's contracting force acts not only on the earth's solid matter but on its gaseous envelope also, and in the latter case with much more powerful results. So that any surface exposed to the sunshine is exposed also to a column of air undergoing contraction or pressure on the part of the sun

and as this compressed air must give out its heat if it is in contact with any body at a less tension, it gives it out to my hand or to the moisture exposed to it, which in the one case feels hot and in the other experiences an effect of of heat, namely evaporation. It is the column of air that is giving out heat, each ray that pierces it squeezes it and squeezes out of it a certain amount of heat which we attribute directly to the sun's influence, while in fact it is only mediately due to it. This I think satisfactorily explains the great and elementary difficulties of my position. I am not aware of any other difficulty which cannot be as easily met, and as I cannot see my way from escaping from the main conclusion at which I have arrived, I have ventured, paradox though it be, to present it for your criticism. To sum up this conclusion in a phrase, I hold that the earth receives no heat direetly from the sun, the sun only supplying the contractile force which induces terrestrial heat.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 31st, 1874.

ALFRED BROTHERS, F.R.A.S., President of the Section, in the Chair.

The following gentlemen were elected Officers of the Section for the ensuing year:—

President.
ALFRED BROTHERS, F.R.A.S.

Vice-Presidents.
E. W. BINNEY, F.R.S., F.G.S.
JOSEPH BAXENDELL, F.R.A.S.

Secretary.
G. V. VERNON, F.R.A.S., F.M.S.

Treasurer.
SAMUEL BROUGHTON.

"The Meteorological Theory of Cometary Phenomena," by DAVID WINSTANLEY, Esq.

On the 16th April, 1872, I had the honour of bringing under the notice of this Society the outline of a theory by which I showed, or endeavoured to show, that in the instance of bodies surrounded by abnormally voluminous atmospheres and subject to sudden and violent thermal changes, certain phenomena indistinguishable from the imposing aspects frequently presented by comets must of necessity ensue as the ordinary phenomena of their meteorology. So far as I have seen no criticisms have been advanced disputing the correctness of this deduction. If, therefore, I am able to satisfy you that any particul

comet really was surrounded by an abnormally voluminous atmosphere, that it was subjected to sudden and violent thermal changes, and that it also exhibited the imposing aspects to which I have alluded, I think I am entitled to ask you to regard the phenomena in question as meteorological phenomena, and to regard the meteorological theory as affording a satisfactory explanation of the appearances presented at any rate by that particular body.

The analyses of meteorites falling upon the surface of the earth from the regions of external space have not, it appears, led to the discovery of any new chemical body*, whilst the spectroscope has shown that chemical substances, common enough upon the earth, exist in the solar atmosphere, in the atmospheres of his attendant planets, in the fixed stars themselves, and in the far distant nebulæ. In short, we have direct evidence that whatever quantitative differences there may be, qualitatively the community of matter extends throughout the fields of nature.

This fact at once disposes of those cometary theories which assume the existence of some new chemical body as a necessary agent in the production of cometic phenomena, and it also disposes of those objections to other theories founded upon the supposition that the ordinary operation of physical laws as observed upon the earth may be rendered inoperative in the case of comets by a non-community in their chemical nature.

We have, it is true, very little special evidence as to the chemical constitution of comets. What little we have has been furnished by comparatively insignificant members of the family, in unfavourable circumstances of luminous intensity, and with indications as yet unsatisfactorily interpreted.† Evidence has however been adduced that comets for the most part consist of matter moving between us and

* Guillemin's "Heavens," Eng. trans., page 175.

† See "Nature," Jan. 8th, 1874.

certain distant stars, matter which is picked up and appropriated by our system in its onward progress through the depths of space.* This in itself removes any ground for regarding comets as possible non-partakers in the universal community of matter. The spectroscope has shown however—and this is strictly to the point in the present inquiry—that of whatever ingredients comets may be composed they certainly are not as a class destitute of volatilizable ingredients.† The physical condition of all terrestrial substances—and in view of the community of matter the physical condition also of extra-terrestrial materials is merely a question of temperature and pressure. In short, given a sufficiently high temperature and a sufficiently low pressure and anything will vaporise.

The great periodic comet of our system, the body named after the illustrious Edmund Halley pursues an orbital path of such eccentricity that it is sixty times more remote from the sun in aphelion than in perihelion and is therefore subjected three thousand six hundred times more to the influence of the solar radiance in the latter position than in the former, a circumstance very suggestive of meteorological phenomena on a grand scale, when we consider that those terrestrial changes which to us seem so momentous are merely local circumstances resulting from local variations of presentation to the sun, from which, as a whole, the earth maintains an all but constant distance. The great comet of 1843 came from an aphelion point, at which the sun's apparent magnitude would only be the two hundredth part of one degree, and the influence of his light and heat one ten thousandth part of that which we enjoy. But this same body in its perihelous passage almost grazed the very surface of the sun approaching the photosphere within one-seventh of the solar radius,‡ and being there exposed to the terrific

* See "Nature," Jan. 22nd, 1874, p. 239.

† See "Roscoe's Spectrum Analysis," p. 290.

‡ Herschel's "Outlines," tenth edit., p. 400.

glare which the earth would feel were it illuminated by 47,000 suns instead of one. A far inferior degree of heat has been found to melt the most refractory rocks and dissipate materials of prodigious fixity, and there can be no doubt that this comet acquired "a temperature high enough to convert even carbon into vapour."*

The *a priori* evidence is complete. There can be no doubt the comet of 1843 possessed an atmosphere, and the only question now is one of its extent. I believe the actual depth of the terrestrial atmosphere is still unknown, though it is estimated as extending to a distance from the surface of more than seventy miles.† Were the mass of the condensed portion of the earth by any means diminished so as to lessen the coercive power exercised thereby over its gaseous parts, the volume of the latter would, according to Herschel, be increased in a proportion exceeding that in which the central mass had been diminished. The mass of comets is so small as to have furnished, so far as I can find, no data for its determination in any individual case. Whatever mass we may suppose the comet of 1843 to be possessed of consistent with this fact, it is clear its atmospheric limits must have been prodigiously remote from the solid matter of the nucleus. Indeed if we consider the recession resulting from increased distance from the central gravitating mass, no volume of atmosphere containable within the known extension of our system would seem too great to be attributed thereto.‡ The deduction from this is that the phenomena resulting to a body of extended atmosphere and subjected to violent thermal change are phenomena which

* Roscoe's "Spectrum Analysis," p. 352.

† Herschel's "Outlines," tenth edit., p. 711.

‡ "Newton, with the view of illustrating this point, calculated that if a globe of common atmospheric air one inch in diameter was expanded so as to have an equal degree of rarity with the air situated at an elevation above the earth's surface equal to the earth's semidiameter, it would fill the whole planetary regions as far as Saturn."—Grant's "History of Astronomy," p. 298.

should have been exhibited in an exceptional degree by the comet of which we speak. And all accounts agree that it presented "a stupendous spectacle." Its tail stretched to a hitherto unheard of distance, and so brilliant an object was this body that it could be seen by daylight and in the immediate neighbourhood of the sun.

It is however only on the gratuitous assumption that comets are composed of the most refractory materials in the universe that any close proximity to the sun is needed as an argument in favour of their having atmospheres. Admitting the community of matter and considering the rigorous cold they must experience in aphelion, it is but reasonable to suppose that as a rule the production of their atmospheres by the evaporation of their nucleii is commenced at distances from the sun even greater than the distance of the earth, and there is plenty of evidence to corroborate this view. The great comet of 1811 presented irrefragable evidences of the existence of a transparent atmosphere certainly several thousands of miles in extent,* and yet its nearest distance from the sun exceeded the distance of the earth ! whilst during the last return to perihelion of Halley's comet in 1835 Sir John Herschel observed appearances which satisfied him that the nucleus of the comet was "powerfully excited and dilated into a vapourous state by the action of the sun's rays, escaping in streams and jets at those points of its surface which oppose the least resistance, and in all probability throwing that surface or the nucleus itself into irregular motions by its reaction in the act of so escaping."† Yet the perihelion distance of this comet was as much as 0·58 of the mean distance of the earth.

The close approximation to identity observed in the orbital

* See observations of Dr. William Herschel in Phil. Trans., vol. 102, pp. 134 to 136.

† Herschel's "Outlines," p. 382.

paths of certain comets and certain meteoric streams is a matter of too serious moment to be passed over without attention by a cometary theorist. The physical connection which it indicates is one to be accounted for. It may be, firstly, that meteors are the uncollected elements or particles from which comets are built up, or secondly, that meteor streams and comets are identical,* or thirdly, that by disintegration and dispersion the constituents of a comet have become a meteor stream.

So far as I have found no evidence has been adduced that comets aggregate material in their journeys round the sun whilst their supposed identity with meteor streams is merely an hypothesis built upon no observation and failing to explain with clearness any of their distinguishing phenomena. There is however evidence enough that sub-division is not uncommon amongst bodies of the cometic class.† Bi-partition is recorded in the Chinese annals four centuries before the birth of Christ, whilst of modern evidences of disintegration the separation of Biela's comet into two in 1846 places the phenomena of sub-division beyond the reach of doubt. The fact of Biela's total loss and the existence of a meteor stream along its former path seem to establish complete disintregation as a known result and a meteor stream as an observed effect. During the apparition of Donati's comet in 1858 an appearance was remarked in connection with the tail which Bond considered due to a quantity of scattered and abandoned gas.‡ But according to that theory which it is the object of this paper to advance the disintegration of cometic bodies is not a matter for surprise. For considering the enormous distances to which according to that theory some portion of the cometic matter is removed and the manner in which it is dispersed it is

* See "Nature," vol. 4, p. 268.

† See "Kirkwood on Disintegration." Nature, vol. 6, p. 148.

‡ "Annals Harvard College," vol. 3, p. 158—60.

quite inconceivable that it should ever be wholly re-absorbed. When its vapourous condition ends a flight of meteors must result whose densest part will lie along the comet's former path.

It was a favourite speculation of the elder Herschel* that in their perihelion passage comets lose some portion of their more elastic matter, so that what remains behind assumes a condition of greater consolidation, whilst other astronomers have believed that at each return a comet loses something of its former splendour.† Upon whatever evidence these beliefs may rest, the circumstances which they indicate are a necessary sequence if the theory here advanced be true.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 16th, 1874.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section in the Chair.

Mr. BAILEY brought under the notice of the Section a method which he had used for some years of exhibiting slides under microscopes, the audience being seated. This was described and explained, and it was proposed that the Council be requested to take the matter into consideration with a view to its being put into use at the meetings of the Section.

Mr. T. S. PEASE exhibited and explained a series of slides which he had brought for examination, showing the internal anatomy of the cockroach.

* "Observations at the Cape," p. 411.

† Smyth's "Cycle," vol. 1, p. 235.

March 16th, 1874.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section in the Chair.

Mr. HURST laid on the table some of the expensive Natural History works it had been decided to purchase with the fund given by the Manchester Natural History Society.

Mr. SIDEBOTHAM exhibited an old book cover of the date 1683, mined by a very rare beetle, *Hypothenemus eruditus*. This species was discovered more than 40 years ago by Professor Westwood and named by him, since which time no other specimen has been seen until this year, when it was met with by Mr. Janson. Mr. Sidebotham met with several specimens in this old book cover, one of which was living.

Mr. Sidebotham also mentioned that during the frost last week he noticed the steam from an engine at London-road Station speedily converted into snow, and on examination of the crystals with a pocket lens found them to consist of unusual forms of the most simple description, equilateral triangles, reels with triangular ends, &c., and a total absence of all wheel-like forms.

Annual Meeting, April 21st, 1874.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

The following Report of the Council was read by one of the Secretaries:

The Treasurer's Account for the past year shows that the general balance has increased from £407 1s. 4d. on the 31st of March, 1873, to £435 13s. 1d. on the 31st of March last. It must however be remarked that this increase is due to the fact that no expenditure has been incurred during the year for binding books, in consequence of the Hon. Librarian having been unable to give his attention to the business of the Library as heretofore.

The number of ordinary members on the roll of the Society on the 1st of April, 1873, was 169, and eleven new members have since been elected; the losses are, deaths, 4; resignations, 3; and defaulters, 2. The number on the roll on the 1st of April instant was therefore 171. The deceased members are Thomas Carrick, Peter Higson, Thomas Turner, and Frederick Crace-Calvert.

Mr. Thomas Carrick was born at Rockcliffe House, five miles from Carlisle, on the 12th of April, 1816. He was the eldest son and third child of David Carrick, Jun., banker, Carlisle, and Sarah his wife, *née* Brockbank, of Ulverstone. When three years old his life was nearly lost by fire; his clothes were ignited in nursery play, and he was severely burned. For many succeeding years he was a suffering and delicate boy, naturally shy and diffident, quiet and fond of books, and not joining much in the active

sports of boys of his own age. His father died in 1821. In 1825 and 1826 he occasionally attended a mixed school in the village of Stanwix, Carlisle, taught by a master in reading, writing, and arithmetic. In December, 1826, he was placed at a boarding school of the Society of Friends, at Wigton, Cumberland, and remained there till October, 1829. After an interval at home he was two years at the late Samuel Marshall's boarding school at Kendal. Mr. Marshall was interested in his pupil, and sent specimens of his writing and drawing to William Johnson and Sons, then land surveyors in Manchester. These specimens, with Samuel Marshall's recommendation, procured him a favourable entrance as an apprentice into Wm. Johnson and Sons' office, in 1832, and to their honour they received him without premium because his mother was a widow. At the close of his apprenticeship he took offices in Brown Street on his own account as land surveyor, and was much engaged on railways by C. F. Cheffins, of London. His home was with the late John B. Brockbank's family, Salford, and at J. B. B.'s death the partnership of Carrick and Brockbank commenced. He was a most dutiful son to his widowed mother up to her death in 1844, and at all times a considerate and loving brother.

Mr. Carrick became a member of the Literary and Philosophical Society in January, 1857; he was elected treasurer on the 6th of October, 1868, and continued to fill this office till the time of his death, May 27th, 1873. He contributed the following papers to the Society:

Feb. 22, 1859.—"On the grouping of unexplained Cosmical Phenomena."

Oct. 13, 1859.—"On Orbit Inclination."

Dec. 8, 1859.—"On the Sun's Orbit plane."

Feb. 2, 1860.—"On the Moon's Orbit plane."

Oct. 11, 1860.—"On the Atomic Constitution of Water and Ice."

Apr. 30, 1863.—"On the wave of High Water: a new Theory of Tides."

In this paper he certainly accounted for many tidal phenomena, and brought together many curious facts gleaned from a wide circle of reading. His views of cosmical phenomena were original, and he frequently took part in discussions from his own peculiar point of view.

Mr. Thomas Turner, F.R.C.S., was born at Truro, the 23rd August, 1793. His father, Mr. Edmund Turner, was a banker in that town. His mother was descended from an old Cornish family named Ferris. Mr. Thomas Turner was the youngest of three sons. The eldest, Edmund, succeeded his father as a banker, and became a member of Parliament, representing his native town in the Liberal interest for 20 years. The second son, Charles, a lieutenant in the East India service, was killed by a sunstroke in India. There were also two sisters older than Mr. T. Turner, both of whom are dead. Educated at Truro Grammar School, he left for Bristol, in 1811, to be apprenticed to Mr. Duck, who was surgeon to St. Peter's Hospital in that town. Mr. Turner left Bristol for London in 1815, and entered as student under Sir Astley Cooper at the United Hospitals of Guy's and St. Thomas's. He passed at the Royal College of Surgeons and Apothecaries' Hall in 1816. After this he studied in Paris, and became acquainted with most of the eminent men who flourished in the Parisian schools at the time. He left Paris in 1817, and it was through the influence of relatives then residing in Manchester that he was appointed resident house surgeon to the Manchester Workhouse. He remained there until the autumn of 1821, when he commenced practice in Piccadilly, opposite the Royal Infirmary.

Mr. Turner was appointed secretary to the Manchester Natural History Society in 1821, and continued for very many years to take an active interest in its progress. The Royal School of Medicine and Surgery, heretofore in Pine Street, and which Mr. Turner has lived to see united to

that noble academical establishment, the Owens College, owes its first complete organisation to Mr. Turner. In the autumn of 1822 he commenced his first course of lectures. They were given at the rooms of the Literary and Philosophical Society, in George Street, on the anatomy, physiology, and pathology of the human body. These lectures Mr. Turner regularly continued till October, 1824, when he established the Medical School, with the object of enabling local practitioners to study their profession completely without the necessity of going to London. Dr. Dalton became the lecturer on chemistry at Mr. Turner's Institution.

In 1831 Mr. Turner was elected surgeon to the Manchester Royal Infirmary, the duties of which office he zealously performed for 25 years, when he resigned, and was appointed honorary consulting surgeon. This position he held to the last. In 1843 he was appointed honorary professor of physiology to the Royal Institution, when he annually delivered a course of lectures—the last course being delivered 12 months ago. Mr. Turner has also been honorary surgeon to the Manchester Deaf and Dumb School from its foundation till now. Of the Infant Deaf and Dumb School he was the chief promoter, and he laid the foundation stone of the building at Old Trafford in 1859. He also originated the Manchester and Salford Sanitary Association in 1851, and was always one of its most earnest supporters. His loss will be very widely felt, from the systematic beneficence which he practised as a medical man.

Mr. Turner was made Fellow of the Royal College of Surgeons of England in 1843, and elected to the Council of the College in 1866. He resigned his seat there last July. In addition to those already enumerated, Mr. Turner held the positions of Fellow of the Linnæan Society, and honorary member of the Harveian Society, London. He was the author of "Outlines of Medico-Chirurgical Science;"

"Observations on Aneurism and Hæmorrhage," "Provincial Medical Education," "Treatise on Dislocations of the Astragalus and Injuries of the Foot," "Retrospect of Anatomy and Physiology," and other publications.

Mr. Turner married, in 1826, Anna Mary, daughter of Mr. James Clarke, of Needham, near Newport, Isle of Wight. One of Mr. Turner's sons is a clergyman at Stockport; another a lieutenant in the army. One daughter, married, is resident in Shropshire. The youngest daughter was her father's devoted nurse at home. His illness lasted many weeks, during which there was no hope of his recovery. He was in his 81st year.

Dr. Frederick Crace-Calvert, F.R.S., was born near London on the 14th of November, 1819.

In the year 1835, when 16 years of age, he left London and went to France, where he commenced the study of chemistry under the celebrated chemist Gerardin, at Rouen, and continued with him for two years. At the expiration of this time he went to Paris, and carried on his studies at the Jardin des Plantes, the Sorbonne, Collége de France, and École de Médecine, his attention being principally given to the Natural Sciences.

About the age of 21 he was appointed to manage the well known works of Messrs. Robiquet & Pelletici, where the manufacture of pure chemicals and pharmaceutical products is carried on. This position, however, he soon vacated on being offered that of "Démonstrateur de Chimie Appliquée," under the eminent chemist Chevreul, and here he remained from 1841 till 1846, when he left France. From the former date his career as a chemist began and continued with untiring energy during the succeeding 32 years.

He published his first paper, "Sur l'extraction de quinine et cinchonine," in September, 1841.

In 1843, in conjunction with M. Ferrand, he elaborated an interesting paper on the analysis of gases enclosed in

some organs of plants, the gases being taken from the same plants at different times of the day and year to demonstrate the action of the sun's rays. This paper is entitled "Memoir sur la Végétation," and may be found in Volume V. of "The Comptes Rendus." In the following year the diseases of beer engaged his attention, and some interesting facts were embodied by him in a paper read to the Société de Pharmacie, "Sur la fermentation visqueuse de la bière."

From 1843 till the time of his leaving France he was engaged in a research on some compounds of lead which first brought him into note. One of the papers consequent on this may be found in the "Comptes Rendus" of 1843, entitled "Procédé au moyen duquel on obtient un protoxyde de plomb cristallisé et ayant la couleur du minium."

In 1844, he wrote "On the presence of indigo in the Orchidaceous plants;" in 1846, "On the preparation of Calomel on the large scale;" and, in the same year, a compilation of facts relating to the properties of animal black.

On returning to England at the latter end of 1846 he was first appointed to the chair of the honorary professorship of chemistry at the Royal Institution, and afterwards to that of lecturer on Chemistry at the School of Medicine in Pine Street, Manchester.

In 1847 he published a paper "On Bleaching Powders," and in 1848 one "On the Bleaching of Cotton and Flax."

About this time Dr. Calvert gave a long series of lectures on his favourite subject at the Athenæum, Manchester, "The Application of Chemistry to Manufactures." These were recorded in the daily papers.

During the following years many other subjects engaged his attention, but we may notice the following publications as some of the results of his labours.

In 1849, "Process for the Preparation of Chlorates, particularly the Chlorate of Potash."

In 1851 "On the Oxides and Nitrates of Lead."

In 1854 "A case of Poisoning by the Sulphate of the Protoxide of Iron."

In 1855 "On the Adulteration of Tobacco."—"On the Action of Organic Acids on Cotton and Flax Fibres."—"On the Actions of Gallic and Tannic Acids in Dyeing and Tanning."

In 1856 "On the Solubility of Sulphate of Baryta in different Acids."—"On the Purification of Polluted Streams."

About this time he commenced an enquiry, in conjunction with Mr. Richard Johnson, on the physical and chemical properties of different alloys. The publications resulting from these investigations were:

In 1858 "On the Hardness of Metals and Alloys."—"On the Conductibility of Metals and Alloys."—"On the Chemical Changes which Pig Iron undergoes in its Conversion into Wrought Iron."

In 1861 a series of papers "On the Expansion of Metals and Alloys."

In 1862 "On the Composition of a Carbonaceous Substance existing in Grey Cast Iron."—"On the Employment of Galvanised Iron for Armour Plated Ships."—On the Conductibility of Heat by Amalgams."

In 1863 "On the Preservation of Iron Plated and other Ships."

The interest he took in the preservation of ships from the action of sea water never ceased; many unrecorded experiments were carried on by him at intervals on this subject till the last days of his life.

In 1865 Mr. Richard Johnson and he published "On the Action of Sea Water on certain Metals and Alloys," and in 1866 "On the Action of Acids on Metals and Alloys."

In 1870, two papers appeared by Dr. Calvert, one "On the Composition of Iron Rust," the other "On the Oxidation of Iron," and a third on the same subject in 1871.

In 1858 we find a publication of his "On a New Method of preparing Hydrochloric Acid."

In 1859 "On the Analyses of Wheaten Flours."—"Influence of Science on the Arts of Calico Printing."—"On Starches: the purposes to which they are applied, and improvements in their Manufacture."

During this year, his attention was called by the late Dr. Ransome to hospital gangrene, and in seeking its cause he was led to investigate the compounds produced during putrefaction.

Two papers, descriptive of his results, appeared in 1860, the first "On Products of Putrefaction." The second "On New Volatile Alkaloids given off during Putrefaction."

He continued, during the following two or three years, with these researches, and had collected about an ounce of a precipitate, produced by combining the gaseous products of putrefaction with platinum, by passing the gases emitted by the putrefying meat through bichloride of platinum, by means of aspirators, during many months. This accumulation of precipitate was unfortunately destroyed before its examination could be completed, through the carelessness of one of his assistants, which caused him much regret ever afterwards.

In 1861 he wrote "On Improvements in the Manufacture of Coloring Matters," and "On the Chemical Composition of Steel." A report followed "On the Action of Water supplied by the Manchester Corporation on Lead of different kinds," in connection with the Manchester Sanitary Association.

In 1862 he gave a series of lectures to the Society of Arts, "On the Improvements and Progress in Dyeing and Calico Printing since 1851;" in 1864 "On Chemistry as applied to the Arts;" in 1866 "On Discoveries in Agricultural Chemistry," and "On Discoveries in the Chemistry of Rocks and Minerals."

These were the beginning of the Cantor lectures, which are now continued every year by different lecturers.

In this same year we find another paper by him, "On Wood for Shipbuilding."

In 1863 he patented and worked at his process for the separation of sulphur from coke, by use of common salt, for the purpose of the manufacture of iron of superior quality.

The following is a list of some of his further publications:

In 1865 "On the Action of Silicate and Carbonate of Soda upon Cotton Fibres."—"On the Crystallized Hydrate of Phenic Alcohol."

In 1866 "On the Hydraulicity of Magnesian Limestone." "On the Preparation of Acetylene."

About this time he interested himself with the properties of phenic or carbolic acid, and being satisfied of its valuable disinfecting properties, built works for its manufacture, and to him belongs the honour of having first brought it in a pure state into commerce.

In 1867 he wrote papers "On Oxidation by means of Charcoal."—"On the Presence of Soluble Phosphates in Cotton Fibres, Wheat, and other Seeds," and five articles "On the Synthesis of Organic Substances."

In 1867 "Carbolic or Phenic Acid and its Properties" (three articles).

In 1869 "Presence of Soluble Phosphates in Seeds."—"Preparation of Nitrogen."

In 1870 "Testing Petroleum."

In 1872 "Sulphur in Coal and Coke;" and papers on "Protoplasmic Life;" "Vitality of Disease Germs," &c. Part of the latter series remains yet unpublished.

In concluding the list of Dr. Crace-Calvert's various researches we may mention that, besides the above, many others were made by him, but their unfinished state does not justify publication. Among these may be mentioned one on "Light," which cost him much labour, and one "On the Action of different Gases on each other under Enormous Pressures."

He was a Fellow of the Royal Society, of the Chemical Society, and of many other societies both at home and abroad.

Dr. Calvert showed remarkable devotion to the science he studied, and his knowledge of its literature was such as very few have attained, and such also as could only be obtained by a most unusual amount of reading, accompanied with strong interest, and in all probability much pleasure. He showed this knowledge more in the departments referring to industry, and, as might be expected, he intended to give his experience to the public in a more convenient form than his lectures presented. One of these works, that relating to "Colours other than Aniline," was nearly completed—if not entirely so—before his last illness, and is expected to be published shortly. Whilst rather exhausted with this work, added to the attention required for the manufactures in which he was engaged, he was chosen as one of the jury of the Vienna Exhibition. The summer of 1873 was sultry and unpleasant, and other causes may have operated to make it unhealthy; but whatever the reason or combination of reasons may have been (and we cannot doubt that work and anxiety contributed), the result was that Dr. Calvert returned in a very enfeebled state, and a few days after his arrival in Manchester was seized with a fatal illness, which terminated on the 24th of October of the same year.

He was a firmly built man, of middle height, and apparently of unusual strength. His hair was dark, and he seemed to be younger in constitution than his years indicated. His manner was animated, and he had great pleasure in communicating information. It is not attempted in this notice to say to what extent his writings have contributed to the advance of science or the knowledge of manufactures, but in the latter department it is certain that his influence was widely felt, and his friendly disposition enabled him

to become a frequent medium of communication between scientific men in England and in France, in both of which countries he felt equally at home. With these combined characteristics the position he made for himself was peculiar, and of its importance we may judge partly by the fact that, although one to which many would be glad to attain, it is not yet filled up.

In accordance with the resolution passed at the last annual meeting the Council has entered into an arrangement with the Manchester Geological Society to afford accommodation for its meetings and room for its library, &c., the Geological Society to pay an annual rent of £25, and also a further sum of £5 per annum for the services of the Keeper of the Rooms.

Steps have been taken for procuring the Incorporation of the Society under the provisions of the "Companies Acts," but a question having arisen whether it may not be necessary to alter the rules of the Society, it has been thought desirable to obtain counsel's opinion on this point before proceeding further in the matter.

An application having been received from the "Scientific Students' Association" for permission to hold its meetings in the rooms of the Society, a resolution will be submitted this evening for the approval of the members, authorising the Council to negotiate the terms and conditions of an arrangement with the Association.

The following papers and communications have been read at the ordinary and sectional meetings of the Society during the present session:—

October 7th, 1873.—"Atmospheric Refraction and the last rays of the Setting Sun," by David Winstanley, Esq.

October 13th, 1873.—"On Specimens of Carex punctata," by Charles Bailey, Esq.

October 14th, 1873.—"Mean Monthly Barometric Readings at Old Trafford, Manchester, from 1849 to 1872," by G. V. Vernon, F.R.A.S., F.M.S.

"Results of Meteorological Observations taken at Langdale, Dimbula, Ceylon, during the years 1868—72," by Edward Heelis, Esq. Communicated by Joseph Baxendell, F.R.A.S.

October 21*st*, 1873.—"On the Relative Work spent in Friction in giving Rotation to Shot from Guns rifled with an Increasing, and a Uniform Twist," by Professor Osborne Reynolds, M.A.

November 4*th*, 1873.—"On the Bursting of Trees and Objects struck by Lightning," by Professor Osborne Reynolds, M.A.

"On some Roman and Celtic Antiquities," by the Rev. W. N. Molesworth, M.A.

"On Vitrified Forts," by Dr. R. Angus Smith, F.R.S., V.P.

November 10*th*, 1873.—"Remarks upon the British locality for Lobelia urens," by J. C. Melville, M.A., F.L.S.

"On Lymexylon Navale," by Joseph Sidebotham, F.R.A.S.

November 18*th*, 1873 —"On the Bursting of Trees and Objects struck by Lightning," by Professor Osborne Reynolds, M.A.

"On the Colour of Nankin Cotton," by Edward Schunck, Ph.D., F.R.S., V.P.

"An Improved Method for preparing Marsh Gas," by C. Schorlemmer, F.R.S.

December 2*nd*, 1873.—"On a Remarkable Thunderstorm at Brockham Green, Surrey, on the 7th November, 1873.

December 8*th*, 1873.—"On a Collection of Shells from the Worden Gravel Pit, near the Leyland Station, near Chorley," by R. D. Darbishire, F.G.S.

December 16*th*, 1873.—"Method of Construction of a New Barometer," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., President.

"On the Explosion of Water," by C. Piazza Smyth, F.R.S., Astronomer Royal of Scotland.

"On the Destruction of Sound by Fog and the Inertness of a Heterogeneous Fluid," by Professor Osborne Reynolds, M.A.

"The Chemical Constitution of Bleaching Powder," by C. Schorlemmer, F.R.S.

December 30th, 1873.—"On the Rapid Growth of the Bar at the Entrance into the Queen's Channel, Liverpool," by E. W. Binney, F.R.S., F.G.S., V.P.

January 13*th*, 1874.—"On Further Improvements in a

Mercurial Air Exhauster," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., President.

"On the Influence of Acids on Iron and Steel," by William H. Johnson, B.Sc.

"On Crystalline Sublimed Cupric Chloride," by S. Carson (Student in the Laboratory of the Owens College). Communicated by Professor H. E. Roscoe, F.R.S.

"Memorandum on Brown-stapled Cotton," by Major R. Trevor Clarke. Communicated by Dr. E. Schunck, F.R.S., V.P.

"On the Graphical Representation of the Movements of the Chest Wall in Respiration," by Arthur Ransome, M.D., M.A.

January 19th, 1874.—"On the Similarity of Certain Crystallised Substances to Vegetable Forms," by Joseph Sidebotham, F.R.A.S.

January 27th, 1874.—"On a Source of Error in Mercurial Thermometers," by Thomas M. Morgan, Student in the Laboratory of Owens College. Communicated by Professor H. E. Roscoe, F.R.S.

"Notes on Fossil Lithothamnia (so-called Nulliporæ)," by Arthur Wm. Waters, F.G.S.

February 3rd, 1874.—"On the Theory of the Tides," by David Winstanley, Esq.

"Rainfall at Old Trafford, Manchester, in the year 1873," by G. V. Vernon, F.R.A.S., F.M.S.

February 10*th,* 1874.—"The Northern Range of the Basques," by W. Boyd Dawkins, M.A., F.R.S., F.S.A.

February 24*th,* 1874.—"On the Drainage of Churchyards and Burial Grounds," by E. W. Binney, F.R.S., F.G.S., V.P.

"On the Effect of Acid on the Interior of Iron Wire," by Professor Osborne Reynolds, M.A.

"On the Movements of the Chest in Respiration," by Dr. Ransome, M.A.

March 3rd, 1874.—"Results of Rain-gauge Observations at Eccles, near Manchester, during the year 1873," by Thomas Mackereth, F.R.A.S., F.M.S.

March 10*th,* 1874.—"On Professor Renault's Memoir on Specimens of Fossil Plants of the Genus Myelopteris," by E. W. Binney, F.R.S., F.G.S., V.P.

"Further Observations and Experiments on the Influence of Acids on Iron and Steel," by William H. Johnson, B.Sc.

"On Dr. Schliemann's Excavations and Discoveries on the site of Troy," by R. D. Darbishire, F.G.S.

"Results of Certain Magnetic Observations made at Manchester during the year 1873," by Professor Balfour Stewart, LL.D., F.R.S.

March 16*th*, 1874.—"On Some Specimens of Hypothenemus Eruditus," by Joseph Sidebotham, F.R.A.S.

March 24*th*, 1874.—"On Some of the Perplexities which the Art and Architecture of the Present are preparing for the Historians and Antiquarians of the Future," by the Rev. Brooke Herford.

"A Few Observations on Coal," by E. W. Binney, F.R.S., F.G.S., V.P.

March 31*st*, 1874.—"The Meteorological Theory of Cometary Phenomena," by David Winstanley, Esq.

April 7*th*, 1874.—"On the Corrosion of some portions of the Cast Iron Roof of the Salford Station of the Lancashire and Yorkshire Railway," by E. W. Binney, F.R.S., F.G.S., V.P.

"On the Action of Nascent Hydrogen on Iron," by William H. Johnson, B.Sc.

"Does the Earth receive any Heat directly from the Sun?" by Henry H. Howorth, Esq.

April 13*th*, 1874.—"On the Introduction of Planorbis dilatatus from America into Lancashire," by Thomas Rogers, Esq.

Several of these papers have already been printed in the current volume of Memoirs, and others have been passed by the Council for printing.

Although the system of electing sectional Associates has not yet met with the success which its promoters anticipated, the Council consider it desirable to recommend that it be continued during the ensuing year.

The Librarian reports that having been unable to give much personal attention to the working of the library, the Council found it necessary to secure some paid help to bring up many arrears, and prepare the parcels of Memoirs and

Proceedings now being distributed to the honorary and corresponding members, and to the various learned bodies with whom the Society exchanges it transactions. These parcels are now on the point of being dispatched to their various addresses. No progress has been made in the binding of the books, and the number needing to be bound is now so large as to demand early attention, prompt and regular binding being of great importance in a public library. Considerable shelf space will be required to provide for the accumulation of works which have no fixed place in the library. The number of Societies holding relations with the Society continues as was reported last year.

SAML. BROUGHTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

FROM MARCH 31ST, 1873, TO MARCH 31ST, 1874.

Dr.

	£ s. d.	£ s. d.	£ s. d.
1873. April 1.—To Balance from last year			505 16 4
1874. Mar. 31.—To ... 18:— 1873, at 42s.	354 18 0		
... 4			
Deceased ... 4 } 16	33 12 0		
In Arrear ... 8		321 6 0	
Arrears—5 at 42s.		10 10 0	
9 Members elected in 1873, April—Dec.	18 18 0		
2 " " 1874, Dec.—Mar.	2 2 0		
	21 0 0		
Less Arrears	1 1 0		
		19 19 0	
11 Admission Fees	23 2 0		
Less Arrears	2 2 0		
		21 0 0	
4 Associates Microscopic Section	2 0 0		
2 " " " Arrears	1 0 0		
		3 0 0	
To *Sale of Publications*			
Proceedings	0 12 10		
Memoirs	0 13 3		
		1 6 1	
To *Sundry Income*:			
Sectional Contributions:			
Microscopic Section	2 2 0		
Physical Section	2 2 0		
		4 4 0	
To Bankers' Interest		15 12 9	
Total Income for Year			396 17 10
To Interest on £1,500 from November 20th, 1872, to December 25th, 1873, Natural History Society, less Tax			64 16 10
			£967 11 0

	£ s. d.
Compound Fund	98 15 0
Natural History Fund	64 16 10
General Balance	435 13 1
	£599 4 11

Cr.

	£ s. d.	£ s. d.
1874. Mar. 31.—By *Charges on Property*.		
Chief Rent ...	13 14 8½	
...	7 0 0	
Duty	2 2 6	
...	5 7 6	
		28 4 8½
By		
...	21 6 11	
...	3 0 11	
...	16 12 7	
		41 0 5
By		
...ooms	57 4 0	
...ne	4 4 0	
...	19 10 1	
Circulars, Stamps	12 15 6½	
		93 13 9½
By *Publishing*:		
Memoirs, Printing and Engraving	44 10 0	
Proceedings, Printing	54 8 3	
Editor of Memoirs and Proceedings	50 0 0	
		148 18 3
By *Library*.		
Periodicals	25 2 11	
Subscription to Ray Society, 2 years	2 2 0	
" Palæontographical Society	1 1 0	
Assistance to Librarian	27 0 0	
		55 5 11
Total Annual Disbursements		365 3 1
By Steel Plate of Dr. Dalton		3 3 0
Total Disbursements		368 6 1
By Balance in Heywood's Bank	597 11 9	
" in Treasurer's hands	1 13 2	
		599 4 11
		£967 11 0

1874, *April 8th*.
SAML. BROUGHTON, TREASURER.
Audited and found correct, April 14th, 1874.
JOSEPH SIDEBOTHAM,
JOHN A. BENNION.

On the motion of Mr. F. NICHOLSON, seconded by Mr. J. A. BENNION, it was resolved unanimously:—"That the Report just read be adopted and printed for circulation among the members of the Society."

On the motion of the Rev. JOSEPH FREESTON, seconded by Mr. D. WINSTANLEY, it was resolved unanimously:—"That the system of electing Sectional Associates be continued during the ensuing session."

On the motion of Mr. C. BAILEY, seconded by Mr. J. BAXENDELL, it was resolved unanimously:—"That the application of the Scientific Students' Association for permission to hold its meetings in the Society's buildings in consideration of a payment to be fixed be acceded to, and the Council be authorised to negotiate the terms and conditions of such arrangement."

On the motion of Mr. E. W. BINNEY, seconded by Dr. SCHUNCK, it was resolved unanimously:—That the thanks of the Society be given to Mr. Bailey for his valuable services as Honorary Librarian."

The following gentlemen were then elected Officers of the Society and Members of Council for the ensuing year:—

President.

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.

Vice-Presidents.

JAMES PRESCOTT JOULE, LL.D., F.R.S., F.C.S., &c.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.
REV. WILLIAM GASKELL, M.A.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.
OSBORNE REYNOLDS, M.A.

Treasurer.
SAMUEL BROUGHTON.

Librarian.
FRANCIS NICHOLSON, F.Z.S.

Of the Council.
ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.
BALFOUR STEWART, LL.D., F.R.S.
ALFRED BROTHERS, F.R.A.S.
REV. BROOKE HERFORD.
CHARLES BAILEY.

The following communication from Dr. JOULE, F.R.S., was read by Mr. BAXENDELL:—

"Will you permit me, in reference to Mr. Howorth's communication to the last meeting, to refer to my papers in the Phil. Trans., 1859, p. 91 and p. 133, in which experiments are described proving what had previously been shown by Sir W. Thomson to be corollaries to the dynamical theory of heat, viz.:—That the heat evolved by substances on being compressed is never exactly equivalent to the force of compression; is generally very different therefrom; while in the case of water taken between the limits 32° and 39° Fahrenheit, cold, not heat, is the result of compression. I cannot therefore, admit the axiom on which Mr. Howorth builds his ingenious theory."

PHYSICAL AND MATHEMATICAL SECTION.

April 28th, 1874.

ALFRED BROTHERS, F.R.A.S., President of the Section, in the Chair.

"On the Ratios and Frequency of Rainfall, deduced from Observations made at Eccles," by THOMAS MACKERETH, F.R.A.S., F.M.S.

A few weeks ago Thomas Glazebrook Rylands, Esq., of Thelwall, near Warrington, was kind enough to draw my attention to the amount of rainfall at that place for the years 1872 and 1873. The amount which fell there in 1872 was 47·52 inches, and in 1873 it was 24·53 inches. These amounts Mr. Rylands considers extremes of rainfall for that district, and he was led to make several comparisons between them. The results of these comparisons show curiously enough that "the amount of excess in rainfall increases with the amount of rain, and therefore that the increase is due mainly to the maximum falls."

In putting the falls of the two years mentioned into ratio they stand thus, 1·940:1. But the number of days of rainfall in the two years stand thus, 1.235:1. Mr. Rylands then introduces into comparison the ratios that exist between other elements; but as those results only apply to his own register, I will not now mention them. I will merely state, having mentioned the rainfall for the two years at that place, that the number of days on which rain fell there in 1872 was 233, whilst in 1873 the number of days was 189. But if the number of days on which rain falls in a wet year is to bear any comparison with those of a dry year the result must be very different, for the ratio

of the amounts of rain and the number of days I have given stand thus, 24·53:47·52::189::366, so that if the number of rainy days at Thelwall in 1872 had any proportion with the rainfall it would have rained there on every day in the year instead of on only 233 days.

Seeing that some information might be derived from a similar consideration of all the years of rainfall I had registered at Eccles, I have made the requisite comparisons and beg now to submit the results to the section.

In the following table I present the rainfall at Eccles for the last thirteen years, in the order of the amount of fall in inches, together with the number of days in each year on which rain fell.

RAINFALL AT ECCLES.

Year.	Amount of Fall in inches.	Number of Days of Rainfall.
1865	27·809	177
1870	30·404	178
1864	30·874	180
1873	31·127	219
1868	32·922	208
1871	33·161	192
1861	33·674	202
1867	35·515	211
1869	35·701	210
1863	36·216	221
1862	37·664	225
1866	43·076	232
1872	48·416	264
Mean	35·119	209

Now, if the extremes of rainfall and number of days on which rain fell, as shown in this table, be placed in comparison, their ratios will stand thus:

27·809:48·416::177:308.

Now the numbers of days on which rain fell, in 1872 was only 264; whereas, if this number had been in propor- to the rainfall, it should have been 308.

From the foregoing table I have deduced by means of the average amount of rainfall, and the average number of days on which rain fell, the ratios of the rainfall of each of the last 13 years, and placed each ratio opposite the ratio of

the number of days of rainfall of each year. They are as follows, column (*b*) representing the ratios of rain-fall, and (*c*) the ratios of the number of days on which rain fell.

Year.	(*b.*)	(*c.*)
1865	·794	·847
1870	·868	·851
1864	·882	·861
1873	·889	1·047
1868	·940	·955
1871	·947	·918
1861	·962	·966
Mean	·897	·920
1867	1·014	1·009
1869	1·020	1·004
1863	1·034	1·057
1862	1·076	1·076
1866	1.230	1·110
1872	1·383	1·263
Mean	1·126	1·086

From the above table it will be seen that the ratios of the first Means are inverse to those of the second Means; and that when the rainfall was below the average there was a relative *increase* of the number of wet days; but when it was above the average there was a relative *decrease* in the number of wet days. So that in wet years, as a rule, more rain falls at a time than in dry years; and we have more rainy days in proportion to the fall in dry years than we have in wet ones.

"The Cause of Solar Heat," by DAVID WINSTANLEY, Esq.

When a body possessing the visible energy of mechanical translation is arrested in its course, that energy, according to the laws of conservation, is not destroyed, but becomes apparent in another form, generally in the form of heat. Should a body receive two equal impulses in diametrically opposite directions at the same time, mechanical translation as a result thereof is impossible, and the energy thus expended, it appears to be agreed, will in the main assume the form of heat. Should a body however receive two equal impulses in directions inclined to each other mechanical translation will ensue, but the value of the energy of visible

motion thus communicated to the body in question is not equal to the sum of the impulses, being, as is well known, justly represented by the diagonal of a parallelogram the length of whose sides is proportioned to the value of the primal impulses. What then in this case becomes of the residue of energy applied and not converted into visible motion? I apprehend it assumes the form of heat. If so in the instance of a body receiving equal impulses in directions inclined to each other at an angle of 120 degrees, the resulting visible motion will just account for the energy of one of these impulses, and the resulting heat for the energy of the other. If then a body having a certain amount of visible energy observable as rectilinear movement have a series of impulses imparted at an angle of 120 degrees in each instance to the line of motion and equal in value to the visible energy of the body at the time of application, the body in question will gain nothing in visible motion through the application of these impulses, but will simply alter the direction of its movement at each application and acquire at the same time an amount of heat of which the impulse is the mechanical equivalent. Adopting this view, it matters not at what angle the impulse may be given, for so long as the figure representing its amount yields with that representing the visible motion of our body a parallelogram whose resultant is equal to the line of visible motion, it follows that the energy of mechanical translation remains unaltered and the energy of the impulse becomes wholly transformed into heat. This condition of things however is what obtains in the instance of a body in planetary revolution in a circular orbit. It is continually receiving impulses through the instrumentality of gravitation at or nearly at right angles to its line of flight, and its energy of visible motion does not increase, whence I infer that the mechanical energy by which its rectilinear movement is destroyed is converted into an equivalent of heat which elevates the

temperature of the planet so far as its conditions of radiation will permit. The aggregate value of the impulses required to make a body describe a circle and return to its primal position and direction without loss or gain of visible energy I have endeavoured to determine graphically by means of the mechanical parallelogram. The result at which I arrive is that the aggregate in question exceeds the momentum of the body to be deviated in the same proportion as the circumference of a circle exceeds its radius, and this irrespective of the circle's size. I have not attempted to calculate the temperature to which the matter of the earth would be elevated by a sudden stoppage in its orbital path, for it appears to me that satisfactory data for doing this do not exist. The number of thermal units to which such a stoppage would give rise if multiplied by 6·28 indicate the amount of heat which upon the present hypothesis the earth annually receives through the instrumentality of solar gravitation. This amount will, I apprehend, amply account for a certain initial temperature of the terrestrial matter, and for that excess of internal heat which appears to have been pretty well made out. It will be evident that any heat which in virtue of this hypothesis may be supposed to fall to the lot of our earth will in some calculable proportion fall to the lot of the other planets also. That proportion I apprehend to be directly as the planet's mass multiplied into its velocity in orbit and divided by its periodic time. Of course this amount of heat will be distributed over the entire matter of the planet, the temperature of which will depend on the capacity for heat of the matter composing it and on its facilities for radiation. These latter will clearly diminish with the comparative diminution of superficies enjoyed by the larger planets and with the extension of their atmospheres. In the case of bodies moving in elliptic orbits the amount of energy which according to this

hypothesis will be transformed to heat in a single revolution will be practically as before, *i.e.*, 6·28 times the planet's visible energy of motion at its mean distance from the sun. There will, however, be this difference between the two cases. In the instance of a body moving in a circular orbit the accession of heat will be a constant quantity in equal increments of time and in all parts of the orbit, whereas in the case of a body moving in an ellipse the accession will be least in approaching the perihelion and greatest in receding from it, inasmuch as the solar gravitation produces an increase of mechanical translation in the former instance and a diminution in the latter. Assuming the theory to be correct this circumstance is one which will greatly mitigate the extremes of heat to which, apart from such a consideration, we should expect cometic bodies to be subject. Indeed, in spite of the enormous distances to which they recede from the sun, the voluminous nature of their atmospheres by which radiation must be diminished, may, in conjunction with this view of the case, afford an explanation of the unexpectedly high temperature which, in the case of these bodies, the spectroscope certainly seems to indicate.

The application of the views herein advanced to an explanation of the cause of solar heat is briefly this:—As the mutual and ever-acting gravitation of the sun and his attendant planets produces in the instance of the central luminary no more acceleration of visible motion than in the instance of the planets themselves, the gravitation must result in the exhibition of some other form of energy, which in the one case as in the other will, I apprehend, be the form of heat. Carrying, however, this idea to the extreme, we should be led to infer that the force of gravitation, which in some circumstances is certainly capable of transference into the form of mechanical translation, and thence into the form of heat, is in others just as capable of transformation in the opposite order. Indeed, if heat is to be regarded as

the individual movement of a multitude of particles whose simultaneous movement is observable as mechanical translation, it is but reasonable to suppose that a force acting upon all, but from the circumstances of the case incapable of producing a simultaneous and concordant movement, will produce an individual and in a sense discordant motion of those same particles. Looking at the matter in this way, any agglomeration of particles whatever must have some initial heat as a result of that gravitation which is unable to evince itself as mechanical translation. And as in our experience gravity is unceasing, this heat so far as we can see should be never ending during the continuance of those conditions which prevent the movement of translation. And a never ending supply of heat must cause an elevation of temperature which will only cease when a point is reached at which the rapidity of radiation equals the rapidity of supply. The more numerous the particles composing the agglomerated body the less become the opportunities of radiation and the more elevated the temperature, which latter attains its maximum in our own system in the instance of the stupendous body which maintains the superficial mundane heat by irradiation from its fires.

It will be seen that the theory here projected places itself in antagonism with the doctrine that "a stone high up" has anything which can be justly termed "the energy of position." But I have already occupied sufficient time without staying now to combat further the doctrine I have named.

To those who have been accustomed to regard gravitation as a property which enables bodies to act where they are not, the present considerations will present difficulties not encountered by others who accept the beautiful, and to my thinking more rational, hypothesis that the simple mechanical movement of infinitesimal particles is the immediate cause of that grand effect the law of universal gravitation.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

April 13th, 1874.

Professor W. C. WILLIAMSON, F.R.S., &c., President of the Section, in the Chair.

Mr. PLANT, F.G.S., exhibited some bones of the extinct Auroch (*Bison priscus*), which had been taken from a deep fissure in the limestone above Castleton, where nearly the whole of the skeleton had been found, together with the bones of the reindeer.

Mr. THOS. ROGERS read a paper "On the Introduction of Planorbis dilatatus" (Gould), a North American fresh water mollusk, which he discovered (June, 1869) adhering to the stones immediately below the surface of the water in the Bolton canal at Pendleton, and in close proximity to the blowing room refuse discharge, and warm water discharge from the engines of Messrs. Armitage's cotton mill. He also afterwards found the same species under similar conditions in the canal adjoining the mills of Messrs. Rylands, at Gorton. After examining all the circumstances under which the mollusk was found (the details of which he placed before the members of the section), he was led to believe that its introduction into this country was by means of American cotton, which had been used for such like war purposes as barricades for steamboats or river defences by the soldiers in the civil war during the presidency of Abraham Lincoln, and which had been accidentally submerged in water and redried with the fry or spawn masses of the Planorbis attached to its fibres previous to its exportation to England, and this ultimately finding its way through the cotton refuse into the canals adjoining the aforementioned mills.

He also remarked the abundance of the beautiful fresh water Zoophyte Plumatella repens, which is found in both habitats of the Planorbis, and on the dead branches of which it seems to find its favourite food. Mr. Rogers said that since the year 1869 (when the mollusk was found in small quantity) it had increased its area of distribution, and multiplied so much as likely to become one of the commonest of our local shells.

Annual Meeting, May 4th, 1874.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

The following report of the Council for the year ending May 4th, was read and passed:

The following papers and subjects have been brought before the meetings during the session:—

November 10*th*, 1873.—"On Lobelia urens," by J. C. Melvill, M.A., F.L.S., &c.

"On the Occurrence of Lymexylon navale in Dunham Park," by Joseph Sidebotham, F.R.A.S.

December 8*th*, 1873.—"On a Collection of Shells from the Drift," by R. D. Darbishire, B.A., &c.

"On Moths and Butterflies captured at Sea 300 miles from the coast of Brazil," by James Linton.

"On an old Microscope made by Benjamin Martin, a celebrated Mathematician of the last century," by J. B. Dancer, F.R.A.S. The microscope was exhibited by Mr. Plant.

January 19*th*, 1874.—"On the Similarity of certain Crystallised Substances to Vegetable Forms," by Joseph Sidebotham, F.R.A.S. Illustrated with drawings, and specimens under the microscope.

February 16*th*, 1874.—"On a Method of Exhibiting Slides under the Microscope to a number of persons seated at a meeting," by Charles Bailey.

"On the Anatomy of the Cockroach," by J. S. Peace.

March 16*th*, 1874.—"On Hypothenemus eruditus," by Joseph Sidebotham, F.R.A.S.

April 13*th*, 1874.—"On the Introduction into Lancashire from America of Planorbis dilatatus," by T. Rogers.

"On a Bone Cave at Castleton," by J. Plant, F.G.S.

Besides these papers various matters of interest have been brought before the members at the meetings, notices of which appear in the Proceedings.

The Council have to report the donation, by Mr. Rideout, of the valuable old microscope which was exhibited to the members in February, 1873, and a figure and description of which appear in the Proceedings of that date.

The Council have had notice of the payment by the Owens College Trustees of the first interest on the £1500 given to this Section by the Manchester Natural History Society, and as soon as this money has been received by our Treasurer, it is purposed to purchase some valuable Natural History works for the library which are much wanted for reference.

From the accompanying statement of accounts it will be seen that the financial position of the Section is satisfactory, the Treasurer having a balance in hand of £43 19s. 9d.

THE MICROSCOPICAL AND NATURAL HISTORY SECTION OF THE LITERARY AND PHILOSOPHICAL SOCIETY, IN ACCOUNT WITH H. A. HURST, TREASURER.

	£	s.	d.
1873.			
To Expenses connected with the Exhibition of the Spence Collection	3	8	6
1874.			
To Parent Society for use of Rooms	2	2	0
,, Charles Simms & Co., Printing Circulars to 18th March, 1874	4	1	6
,, W. Roscoe, Teas and Postages	4	10	2
,, J. E. Cornish, Microscopical Journal to January, 1874	0	16	0
,, Envelopes	0	16	0
,, Check Book	0	2	6
,, Balance at the Manchester and Salford Bank, St. Ann's Street	43	19	9
	£59	16	5

	£	s.	d.
1873.			
By Balance at Messrs. Heywood's Bank	37	13	0
1874.			
By Associates' Subscriptions	3	10	0
,, Members' Subscriptions	17	10	0
,, Interest allowed by Messrs. Heywood	1	3	5
	£59	16	5

Examined and found correct,
(Signed) JOSEPH SIDEBOTHAM,
JOHN BARROW.

(Signed) H. A. HURST, Treasurer.

Mr. SIDEBOTTOM exhibited a cut flower of *Primula Japonica* the result of Hybridisation.

The election of officers for Session 1874–5 was then proceeded with, and the following gentlemen were appointed:

President.
R. D. DARBISHIRE, B.A., &c.

Vice-Presidents.
JOSEPH BAXENDELL, F.R.A.S., &c.
W. BOYD DAWKINS, F.R.S., &c.
W. C. WILLIAMSON, F.R.S., &c.

Treasurer.
HENRY ALEXANDER HURST.

Secretaries.
SPENCER H. BICKHAM, JUNR.
JOSEPH SIDEBOTHAM, F.R.A.S.

Of the Council.
CHARLES BAILEY.
JOHN BARROW.
ALFRED BROTHERS, F.R.A.S.
THOMAS COWARD.
J. COSMO MELVILL, M.A., F.L.S.
THOS. H. NEVILL.
ROBERT B. SMART, M.R.C.S.

The following is the List of Members and Associates:

List of Members.

ALCOCK, THOMAS, M.D.
BAILEY, CHARLES.
BARROW, JOHN.
BAXENDELL JOSEPH, F.R.A.S.
BICKHAM, SPENCER H., Jun.
BINNEY, EDWARD WM., F.R.S., F.G.S.
BROCKBANK, W., F.G.S.
BROGDEN, HENRY.
BROTHERS, ALFRED, F.R.A.S.
COTTAM, SAMUEL.
COWARD, EDWARD.
COWARD, THOMAS.
DALE, JOHN, F.C.S.
DANCER, JOHN BENJ., F.R.A.S.
DARBISHIRE, R. D., B.A.
DAWKINS, W. BOYD, F.R.S.
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HEYS, WILLIAM HENRY.
HIGGIN, JAMES, F.C.S.
HURST, HENRY ALEXANDER.
LATHAM, ARTHUR GEORGE.
MACLURE, JOHN WM., F.R.G.S.
MELVILL, J. C., M.A., F.L.S.
MORGAN, EDWARD, M.D.
MORRIS, WALTER.
NEVILL, THOMAS HENRY.
PIERS, SIR EUSTACE.
RIDEOUT, WILLIAM J.
ROBERTS, WILLIAM, M.D.
SIDEBOTHAM, JOSEPH, F.R.A.S
SIMPSON, HENRY, M.D.
SMART, ROBERT BATH, M.R.C.S.
SMITH, ROBERT ANGUS, Ph.D., F.R.S., F.C.S.
VERNON, GEORGE VENABLES, F.R.A.S.
WILLIAMSON, WM. CRAWFORD, F.R.S., Prof. Nat. Hist., Owens College.
WRIGHT, WILLIAM CORT.

List of Associates.

HARDY, JOHN.
HUNT, JOHN.
LABREY, B. B.
LINTON, JAMES.
MEYER, ADOLPH.
PEACE, THOS. S.
PERCIVAL, JAMES.
PLANT, JOHN, F.G.S.
ROGERS, THOMAS.
RUSPINI, F. O.
STIRRUP, MARK.
TATHAM, JOHN F. W.
WATERHOUSE, J. CREWDSON.

PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

VOL. XIV.

SESSION 1874-75

MANCHESTER:

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1875.

1877. Jan. 24

Legacy of

the Society.

NOTE.

The object which the Society have in view in publishing their Proceedings is to give an immediate and succint account of the scientific and other business transacted at their meetings to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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INDEX.

ERRATA.

Page 6, line 8, for "Odontogigram" read "Odontogram."

„ 6, „ 17, for "indentification" read "identification."

„ 131, „ 1, for "1874" read "1875."

PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 6th, 1874.

Rev. WILLIAM GASKELL, M.A., Vice-President, in the Chair.

"On the Ossiferous Deposit at Windy Knoll, near Castleton," by ROOKE PENNINGTON, Esq., LL.B.

In October, 1870, I was in the Windy Knoll quarry, near Castleton, a place well known to geologists, when I observed a large bone, a tibia, sticking out of some debris overlying the rock. I took it and one or two smaller bones away with me and showed them to Mr. Boyd Dawkins, who named them, and after conversation with him, I decided to explore the place. This could not be done for a long time, as the floor of the quarry was covered with an increasing mass of broken stone for the repair of the turnpike, and the fissure near which the bone was found was so placed that in removing earth from it serious injury would arise to the road stone.

The place was, however, looked after, the permission of the proprietor for an investigation obtained, and from time to time some good specimens were obtained both of bison and reindeer bones. (Mr. Dawkins had made me certain of the nature of the bones.) The day before Good Friday the quarrymen had picked up a number of bones from a fall of earth which had occurred, and placed them in a basket. They forgot to bring them down to Castleton, and on Good Friday, some young men from Manchester carried them off. They repeated their visit once or twice, and did considerable damage by pulling away the soil, so that we had to set a man purposely to watch the place, and preserve it as far as possible

from this miscellaneous intrusion. The specimens thus obtained were carried to Mr. James Plant, who, on the 28th April, 1874, read an account of them to the Manchester Geological Society. One statement which he made then, and which he subsequently repeated in his remarks on a paper read before that Society, by Mr. Aitken, I must very distinctly deny. No bones were ever carted away from the place to any bone-mill, nor we (as will have been seen) ignorant of their nature; the only foundation for the carting statement is the fact that a man, casually employed in the quarry, did take a basketful of bones to his house, but they were immediately recovered, and are now in my collection. I shall have occasion to refer again to Mr. Plant's observations.

The position of the quarry is somewhat remarkable. It is in a small mountain limestone hill, near the extreme north of that tract of limestone which lies between the two ranges of millstone grit and Yoredale hills running S.E. and S.W. from Kinder Scout. The fault separating the mountain limestone from the overlying Yoredale rocks of Mam Tor runs close to the quarry. The water on both sides of the hill runs eastward into the vale of Hope and so into the Derwent. To the west is a valley extending about two miles in length, whence there is no surface outlet, but all the streams disappear towards the south and then turn east, appearing again in the Speedwell mine and flowing out of the Peak Cavern. There is no drainage from this valley into the Mersey, as stated by Mr. Plant, but all the water flows ultimately into the Trent. So far also from there being no appearance in this locality of "swallow holes," as he says, there is an unusually large number in the valley, one in the quarry itself and another very important one close by to the east.

We commenced work at the end of April. The bones observed were in a fissure some little distance up the northern side of the quarry. We soon ascertained that this fissure

was but an opening into a rock basin lying still further to the north. There was no appearance of a cave, and certainly none connected with the fissure has ever existed to the south, that is, where the rock has been quarried away. This basin and the fissure were filled with a reddish coloured loam, such as usually occurs in mountain limestone fissures. Capping this was a quantity of rubbish, derived from previous workings of the quarry. This loam has been described by Mr. Plant as "drifts originally derived from the washing and wearing of the shales and sandstones of the great escarpment of Mam Tor," and as "drifted loam." There is no appearance whatever of the characteristics of drift, or glacial action of any kind. All the fragments included in the loam are angular, unrolled, and of limestone. Not a trace of any foreign rock, even of Yoredale origin, was present.

After clearing out the fissure, we began, in May, a systematic exploration of the basin behind, blowing down the rock which formed its southern wall. We had four men at work for a fortnight, Mr. John Tym superintending. The rubbish at the top contained no pleistocene animals, and the loam in its upper portion afforded but few bones. But at about 4 feet below the surface where we first commenced was a truly wonderful agglomeration of mammalian relics. Bones and teeth of bisons, reindeer, bears, and wolves were turned out in the greatest abundance. Lying as they did in a thick, sticky loam, the work was necessarily slow, as great care had to be taken not to break the bones. The depth of the ossiferous portion of the basin averaged about 12 feet, and about 22 cubic feet were got out. Near the top the specimens were rotten and ill preserved, lower down they were much firmer and more perfect. At the bottom of the fissure and near the sides of the basin, they were welded into a mass with included limestone fragments by stalagmite such as the specimens on the table. This had evidently been derived from the dripping of water charged

with carbonate of lime from the sides. The bones numbered in all many thousands, and ot teeth more than 500 were obtained.

The accumulation of these bones must have been a work of time. It is clear that those encrusted with stalagmite must have lain exposed for some tolerably lengthy period. As to the loam itself, it bears all the characteristics of being formed from the sub-ærial disintegration of the rocks immediately around. Moreover, all the included rocks being angular and of limestone have evidently fallen in from those above. There is no reason to suppose that the washing and wearing of the sandstones and shales of Mam Tor have had anything to do with it; on the contrary, the deposit is just like those in fissures, which are not near the Yoredale rocks, and very unlike the debris from Mam Tor in the Castleton valley. In fact, there is a slope of the limestone towards the place, but certainly there is no trace of the rapid action of water or of any drifting.

I should be inclined to say that this was probably in Pleistocene times a swampy drinking place. It is on the direct tract from the fertile valley of the Derwent (near which I have found traces of these and other Pleistocene animals) to the Cheshire plains. Probably large herds of bisons and reindeer passed the spot, in drinking some would fall in, some would be bogged, others might die in the vicinity and be washed in during rainy weather. The bears and the wolves probably attended to eat up the sickly ones and stragglers, just as such creatures do now in Siberia. Some of the bones and antlers bear marks of gnawing, by what animal I do not offer any opinion, but they no doubt had lain on the surface of the ground when so gnawed.

I should like to call attention to the absence of the rhinoceros, hyæna, cave bear, and other animals at Windy Knoll, and also the diseased character of some of the bones. Also to the absence of the urus (*bos primigenius*). Mr.

Boyd Dawkins at first thought we possessed bones of this animal, but on further comparison found them to belong to the bison, which is, of course, a totally distinct species.

In conclusion attention was drawn to the absence of signs of glacial action in the neighbourhood of Castleton, and to the fact that such signs were only found on the western slope of the western fork of the Pennine chain in Derbyshire, Cheshire, and Staffordshire.

"On some teeth from a fissure in Waterhouses Quarry, in Staffordshire."

Mr. Pennington called attention to some teeth of a bison (*Bos priscus*) from a fissure in a quarry at Waterhouses. The animal had evidently fallen in whilst coming to drink at the river Hamps. It had been erroneously described as an Irish elk.

Mr. Boyd Dawkins, F.R.S., said that the two most abundant animals in the Windy Knoll fissure were the Bison and the Reindeer. It is worthy of remark that the young of the former were out of all proportion to the adult, a fact which implies that the place was haunted by these animals in the summer and early autumn, the number of calves under five months being very considerable, and May being the calving time of the Bison. They were unaccompanied by any other herbivora, the small phalange which I had at first referred to the Roedeer, viewed in the light of the large series discovered by Mr. Pennington, belonging to the former animal.

There were also the remains of the hare, rabbit and water rat.

The other animals which have been discovered consist of the wolf and bear, and these remains are comparatively rare.

There is nothing of any importance to be remarked concerning the wolf, but the ursine remains are of peculiar interest as implying the existence of a new carnivore in

Derbyshire. The mean measurements of the teeth, (molar and canines,) in Mr. Pennington's collection coincide, I may say, almost to a hair's breadth, with the mean measurements of the fossil Grizzly Bear (*Ursus priscus*) which Professor Busk has defined so admirably in his recent memoir on the animals found in the Brixham cave, and leave no room to doubt that the Bear of Windy Knoll belongs to that species or variety. If the Odontogigram of the dentition be compared with that published in the Philosophical Transactions 1873, pl. 47, fig. 8, it will be seen that the agreement is exact.

The Cave-bear, or, *Ursus spelœus*, is also stated by Mr. Plant, (Manchester Geological Transactions, xiii, 130—156), to have been discovered in the Windy Knoll fissure, principally on the fancied resemblance which a sacrum of a young animal bore to a sacrum in the Peel Park Museum, said to belong to *Ursus spelœus*, partly also on the stumps of two teeth, worthless for purposes of specific indentification. I have carefully analysed this evidence, and on comparing the sacrum in question with that of the ox and bear, I believe that it belongs to a young bison, and not to any carnivora. And, further, even if it belong to a bear, there is no evidence as to the species, because the specific characters of that bone in the fossil bears have not yet been ascertained. The researches of Professor Busk, during a long series of years, and my examination of the most important collections of fossil bears in this country and in France, prove that the determination of the species is a point of extreme difficulty, and we are only able to detect characters of specific value in the heads and dentition. On this point I would refer to Professor Busk's memoir, and to the vast collection at Toulouse. The Cave-bear, therefore of Windy Knoll, must be given up, as being based on a faulty determination.

The net result of the examination of the whole group of remains is the conclusion that in the Pleistocene age great herds of bison and reindeer passed up from the valley of the Derwent into the plains of Cheshire, and that they were accompanied by grizzly bears, wolves,

and a few foxes, which ate up the stragglers. The bison, in its migrations, still has the escort of grizzly bears and wolves in the region of the Rocky Mountains; and the reindeer is described, by Admiral Von Wrangel, as being followed by the same animals in Siberia, in its vernal and autumnal migrations.

"On the Extent and Action of The Heating Surface for Steam Boilers," by Professor OSBORNE REYNOLDS, M.A.

The rapidity with which heat will pass from one fluid to another through an intervening plate of metal is a matter of such practical importance that I need not apologise for introducing it here. Besides its practical value it also forms a subject of very great philosophical interest, being intimately connected with, if it does not form part of, molecular philosophy.

In addition to the great amount of empirical and practical knowledge which has been acquired from steam boilers, the transmission of heat has been made the subject of direct inquiry by Newton, Dulong and Petit, Péclet, Joule, and Rankine, and considerable efforts have been made to reduce it to a system. But as yet the advance in this direction has not been very great; and the discrepancy in the results of the various experiments is such that one cannot avoid the conclusion that the circumstances of the problem have not been all taken into account.

Newton appears to have assumed that the rate at which heat is transmitted from a surface to a gas and *vice versa* is *ceteris paribus* directly proportional to the difference in temperature between the surface and the gas, whereas Dulong and Petit, followed by Péclet, came to the conclusion from their experiments that it followed altogether a different law.*

These philosophers do not seem to have advanced any theoretical reasons for the law which they have taken, but have deduced it entirely from their experiments, "à chercher par tâtonnement la loi que suivent ces résultats.†"

* *Traite de la Chaleur,* Péclet, Vol. I., p. 365.

† *Ib.,*-p. 363.

In reducing these results, however, so many things had to be taken into account and so many assumptions have been made that it can hardly be a matter of surprise if they have been misled. And there is one assumption which upon the face of it seems to be contrary to general experience, this is, that the quantity of heat imparted by a given extent of surface to the adjacent fluid is independent of the motion of that fluid or of the nature of the surface;* whereas the cooling effect of a wind compared with still air is so evident that it must cast doubt upon the truth of any hypothesis which does not take it into account.

In this paper I approach the problem in another manner from that in which it has been approached before. Starting with the laws recently discovered of the internal diffusion of fluids I have endeavoured to deduce from theoretical considerations the laws for the transmission of heat, and then verify these laws by experiment. In the latter respect I can only offer a few preliminary results; which, however, seem to agree so well with general experience, as to warrant a further investigation of the subject, to promote which is my object in bringing it forward in the present incomplete form.

The heat carried off by air or any fluid from a surface, apart from the effect of radiation, is proportional to the internal diffusion of the fluid at and near the surface, *i.e.*, is proportional to the rate at which particles or molecules pass backwards and forwards from the surface to any given depth within the fluid, thus, if AB be the surface and *ab* an ideal line in the fluid parallel to AB then the heat carried off from the surface in a given time will be proportional to the number of molecules which in that time pass from *ab* to AB—that is for a given difference of temperature between the fluid and the surface.

This assumption is fundamental to what I have to say, and is based on the molecular theory of fluids.

Now the rate of this diffusion has been shown from various considerations to depend on two things:—

1. The natural internal diffusion of the fluid when at rest.

* *Traite de la Chaleur,* Péclet, Vol. I., p. 383.

2. The eddies caused by visible motion which mixes the fluid up and continually brings fresh particles into contact with the surface.

The first of these causes is independent of the velocity of the fluid, if it be a gas is independent of its density, so that it may be said to depend only on the nature of the fluid.*

The second cause, the effect of eddies, arises entirely from the motion of the fluid, and is proportional both to the density of the fluid, if gas, and the velocity with which it flows past the surface.

The combined effect of these two causes may be expressed in a formula as follows:

$$H = At + B\rho vt, \qquad \text{(I)}$$

where t is the difference of temperature between the surface and the fluid, ρ is the density of the fluid, v its velocity, and A and B constants depending on the nature of the fluid H being the heat transmitted per unit of surface of the surface in a unit of time.

If therefore a fluid were forced along a fixed length of pipe which was maintained at a uniform temperature greater or less than the initial temperature of the gas, we should expect the following results.

1. Starting with a velocity zero, the gas would then acquire the same temperature as the tube. 2. As the velocity increased the temperature at which the gas would emerge would gradually diminish, rapidly at first, but in a decreasing ratio until it would become sensibly constant and independent of the velocity. The velocity after which the temperature of the emerging gas would be sensibly constant can only be found for each particular gas by experiment; but it would seem reasonable to suppose that it would be the same as that at which the resistance offered by friction to the motion of the fluid would be sensibly proportional to the square of the velocity. It having been found both theoretically and by experiment that this resistance is connected with the diffusion of the gas by a formula:

$$R = A^1 v + B^1 \rho v^2, \qquad \text{(II)}$$

* Maxwell's Theory of Heat, Chap. XIX.

And various considerations lead to the supposition that A and B in (I.) are proportional to A^1 and B^1 in (II.)

The value of v which this gives is very small, and hence it follows that for considerable velocities the gas should emerge from the tube at a nearly constant temperature whatever may be its velocity.

This, as I am about to point out, is in accordance with what has been observed in tubular boilers as well as in more definite experiments.

In the Locomotive the length of the boiler is limited by the length of tube necessary to cool the air from the fire down to a certain temperature say 500°. Now there does not seem to be any general rule in practice for determining this length, the length varying from 16ft. to as little as 6, but whatever the proportions may be each engine furnishes a means of comparing the efficiency of the tubes for high and low velocities of the air through them. It has been a matter of surprise how completely the steam-producing power of a boiler appears to rise with the strength of blast or the work required from it. And as the boilers are as economical when working with a high blast as with a low, the air going up the chimney cannot have a much higher temperature in the one case than in the other. That it should be somewhat higher is strictly in accordance with the theory as stated above.

It must, however, be noticed that the foregoing conclusion is based on the assumption that the surface of the tube is kept at the same constant temperature, a condition which it is easy to see can hardly be fulfilled in practice.

The method by which this is usually attempted is by surrounding the tube on the outside with some fluid the temperature of which is kept constant by some natural means, such as boiling or freezing, for instance the tube is surrounded with boiling water. Now although it may be possible to keep the water at a constant temperature it does not at all follow that the tube will be kept at the same temperature; but on the other hand, since heat has to pass from the water to the tube there must be a difference of temperature between them, and this difference will be proportional

to the quantity of heat which has to pass. And again the heat will have to pass through the material of the tube, and the rate at which it will do this will depend on the difference of the temperature at its two surfaces. Hence if air be forced through a tube surrounded with boiling water, the temperature of the inner surface of the tube will not be constant but will diminish with the quantity of heat carried off by the air. It may be imagined that the difference will not be great: a variety of experiments lead me to suppose that it is much greater than is generally supposed. It is obvious that if the previous conclusions be correct this difference would be diminished by keeping the water in motion, and the more rapid the motion the less would be the difference. Taking these things into consideration the following experiments may, I think, be looked upon, if not as conclusive evidence of the truth of the above reasoning, yet as bearing directly upon it.

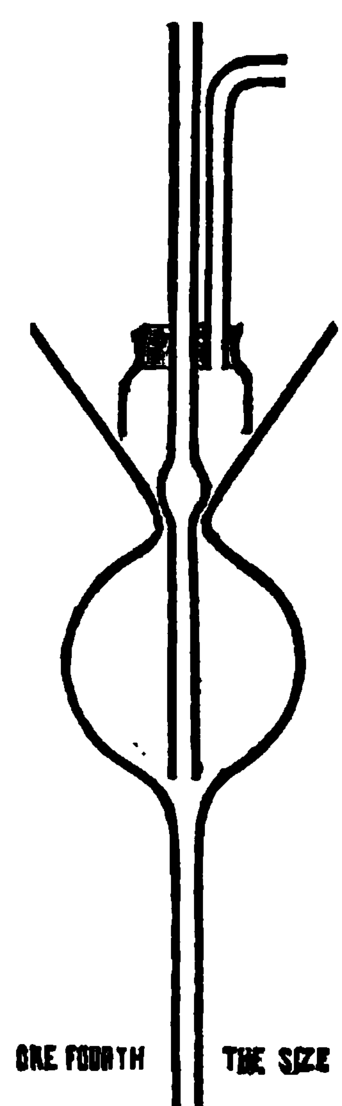
One end of a brass tube was connected with a reservoir of compressed air, the tube itself was immersed in boiling water, and the other end was connected with a small non-conducting chamber formed of concentric cylinders of paper with intervals between them in which was inserted the bulb of a thermometer. The air was then allowed to pass through the tube and paper chamber, the pressure in the reservoir being maintained by bellows and measured by a mercury gauge; the thermometer then indicated the temperature of the emerging air. One experiment gave the following results:—With the smallest possible pressure the thermometer rose to 96° F., and as the pressure increased fell until with $\frac{1}{10}$ inch it was 87°, with $\frac{1}{4}$ inch it was 70°, with 1 inch it was 64°, with 2 inches 60°, beyond this point the bellows would not raise the pressure.

It appears, therefore, (1) that the temperature of the air never rose to 212, the temperature of the tube, even when moving slowest; but the difference was clearly accounted for by the loss of heat in the chamber from radiation, the small quantity of air passing through it not being sufficient to maintain the full temperature, an effect which must obviously vanish as the velocity of the air increased; (2) as

the velocity increased the temperature diminished, at first rapidly and then in a more steady manner. The first diminution might be expected from the fact that the velocity was not as yet equal to that at which the resistance of friction is sensibly equal to the square of the velocity as previously explained. The steady diminution which continued when the velocity was greater was due to the cooling of the tube. This was proved to be the case, for at any stage of the operation the temperature of the emerging air could be slightly raised by increasing the heat under the water so as to make it boil faster and produce greater agitation in the water surrounding the tube. This experiment was repeated with several tubes of different lengths and characters, some of copper and some of brass, with practically the same results. I have not however as yet been able to complete the investigation, and I hope to be able before long to bring forward another communication before the Society.

I may state that should these conclusions be established, and the constant B for different fluids be determined, we should then be able to determine, as regards length and extent, the best proportion for the tubes and flues of boilers.

Dr. JOULE made a further communication respecting his mercurial air pump described in the Proceedings for Dec. 24, 1872; and Feb. 4, Feb. 18, and Dec. 30, 1873. He had successfully made use of the glass plug proposed in the Proceedings for Feb. 4, 1873. This he constructs by blowing out the entrance tube and grinding the bulb thus formed into the neck of the thistle-shaped glass vessel. To collect the pumped gases he now employs an inverted glass vessel attached to the entrance tube and dipping into the mercury in the upper part of the thistle glass.



Ordinary Meeting, October 20th, 1874.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. WILLIAM H. JOHNSON, B.Sc., showed two remarkable pieces of iron cinder from a furnace in which iron is reheated. The samples showed on one side small dark prismatic crystals which appeared to have been formed in a cavity of the cinder as it cooled in the cinder bogie. The reverse side of one of them had formed the wall of a second cavity; its surface was however smooth, black, shining, and studded all over with the sides of oblong jet-black crystals unusually iridescent. He remarked that probably these crystals were Fayalite, an iron chrysolite, a mineral found in the Mourne mountains in Ireland, which is sometimes iridescent, and whose chemical composition is represented by the formula Fe_2SiO_4. They are the more worthy of notice from the rare occurrence of crystals in mill furnace cinder.

E. W. BINNEY, F.R.S., F.G.S., said that in Tome XXII., No. 9, of the Memoirs of the French Academy of Science, MM. B. Renault and Grand' Eury have published a most valuable memoir on the structure of *Sigillaria spinulosa*, and substantially confirmed M. Brongniart's views of the structure of *Sigillaria elegans* published in 1839. Unfortunately the medulla in both these species was destroyed, as is generally the case with specimens of *Sigillaria* as well as the root so long known as *Stigmaria ficoides*. Professor Geoppert and himself many years since described the only two specimens of this fossil which showed structure in the medulla, and both these specimens were looked upon as more than doubtful. Professor W. C. Williamson, F.R.S., in Part 11 of his Memoirs in the Phil. Transactions, p. 215, states: "I have elsewhere called attention to the way in

"which the rootlets of *Stigmaria* have penetrated every-"thing within their reach which was penetrable, and I have "no doubt that in both Professor Goeppert and Mr. Binney's "specimens these supposed medullary vessels were really "Stigmarian rootlets that had found their way into the "interior of the cavity left by the decay of the medulla and "been mistaken for a part of the plant into which they had "intruded themselves." Now in his (Mr. Binney's) Staffordshire specimen described in the *Quarterly Journal of the Geol. Society* for 1850, p. 77, mention is only made of the large vascular bundles found in the axis without calling them vascular or any other vessels. As figured in the plate and described in the letterpress no one could scarcely take them for the radicles of *Stigmaria.* The woody cylinder was one of those having the inner parts of their vascular circle close together and not open as in Professor Goeppert's specimen. It is certainly possible that the large tubes in his specimen may not be in their normal condition and may have been somewhat altered in the process of mineralization, but it is very improbable that they had ever been introduced into the axis after the pith had been removed.

The beautiful specimen figured and described by Goeppert more than thirty years since is very different from his (Mr. Binney's), being much more open in the spaces between the wedges of the woody cylinder, and its central part is enclosed in a *Stigmaria,* shewing the external characters in a most excellent state of preservation, and one of the best that has ever been found. However it might be urged that the vascular bundles in the medulla had been squeezed from their true position into the parts where they are now found, they are certainly not intruded rootlets, as any one who examines the learned author's plate can satisfy himself.

For many beautiful specimens of *Stigmaria,* shewing structure in most of their parts except the medulla from the trap ashes of Scotland, he was indebted to the kindness of Messrs. Wunsch, John Young, and Greive.

He had been so fortunate as to find a specimen of *Stigmaria,* which he now exhibited to the Society, from the

Bullion coal at Clough Head, near Burnley, having the medulla perfectly preserved. It is about two inches in diameter, and shews the inner bark composed of elongated utricles in radiating series and traversed by bell-shaped orifices, containing the bases of the rootlets, the zone of lax cellular tissue, and the woody cylinder of close wedges surrounding the central axis or medulla. Distinct evidence of both the large primary and the small secondary medullary rays is found in the tangential section of the woody cylinder exactly resembling those met with in the fossil plant described by him as *Sigillaria vascularis.* The sharp line of a dark colour separating the vascular cylinder from the central axis is the same in both plants, and the outer portion of the axis is formed of small vascular tubes of hexagonal and pentagonal forms, which gradually increase in size as they proceed inwards, and form something like a medullary sheath enclosing a medulla composed of very small and short barred tubes or utricles in which are mingled large vascular tubes or utricles, the latter being about 15 times the diameter of the former.

The size of these large vascular tubes or utricles in the medulla exceeding anything, so far as his knowledge extended, hitherto observed in fossil plants shews that it was easily decomposed, and thus accounts for the general absence of the medulla in *Sigillaria* and its roots. Every part of this specimen is identical in structure with the plant named by him *Sigillaria vascularis,* so if that is sufficient evidence of the connection of a stem with a root, it must be taken to be the root of that plant. One thing is certain that the large vascular tubes or utricles in the medulla existed in the living plant and are not the intruded rootlets of *Stigmaria.*

Mr. R. D. DARBISHIRE, F.G.S., exhibited and described the Palæolithic (French and English Drift) Implements collected for the Soirée at the Owens College.

Professor BOYD DAWKINS, F.R.S., brought before the notice

of the Society the conditions under which the palæolithic implements are found in the river-strata and in the caves, in association with the extinct mammalia, such as the Mammoth and woolly Rhinoceros. Although the number of flint implements from the river-strata in various collections was very great, yet it is small when viewed in connection with the emormous quantity of gravel removed in their discovery. They are not evenly distributed, but cluster round certain spots. Their discovery in India along with the extinct mammalia proves that man was living, both in Europe and in southern Asia from the Ganges to Ceylon in the same rude uncivilised state, *at the same time* in the life-history of the earth. He also called attention to the art of the hunters of the reindeer and mammoth in the south of France, Belgium, and Switzerland, an art eminently realistic, and by no means despicable, and he inferred from their art and implements and the associated animals that they may be represented at the present day by the Eskimos.

"On a Colorimetric Method of Determining Iron in Waters," by Mr. THOMAS CARNELLEY, B.Sc. Communicated by Professor H. E. ROSCOE, F.R.S.

Of late years the analysis of water has become of such importance that any improvement in the methods employed in that analysis will, it is thought, be acceptable, however small such improvement may be; and it is with this consideration that the following paper is submitted to the Society.

In the determination of heavy metals in water, with the exception of lead, great inconvenience arises from the want of rapid and accurate methods of estimating very small quantities, and it is to remedy this inconvenience in the case of iron that the following method is proposed. Besides accuracy it fulfils both the other requisites, viz. rapidity and the power of determining exceedingly small quantities; for without any evaporation 1 part of iron in 13,000,000 parts of water can be detected and a determination made in less than fifteen minutes; the smallest amount of ammonia

which can be detected by the well known Nessler test, without contraction, being only 1 part of ammonia in 20,000,000 parts of water; and moreover, as water will admit of evaporation without loss of any iron it may contain, the iron which can be estimated may be reduced to almost an infinitely small quantity.

The method consists in the comparison of the blue colours produced by adding to a solution of potassium ferrocyanide, first, a solution of iron of known strength, and secondly, the water in which the iron is to be determined.

The standard solutions and materials required are as follows:—

(1) *Standard Iron Solution.*—This is prepared by weighing out 0·7 grms. of ammonio-ferrous sulphate (= 0.1 grm. Fe), dissolving the water and adding 1cc. of the sulphuric acid; the iron is next oxidised by adding an exact sufficiency of the potassium permanganate solution from a burette and the whole diluted to 1 litre. Of this solution 1cc. =0·0001grm. Fe.

(2) *Solution of Potassium Permanganate.*—This must be moderately dilute, but it is not necessary that it should be of standard strength.

(3) *Standard Nitric Acid*—is prepared by diluting 50cc. of pure strong nitric acid to one litre.

(4) *Potassium Ferrocyanide Solution*—is obtained by dissolving 1 part of the salt in 25 parts of water.

(5) *Strong Sulphuric Acid*—diluted with an equal volume of water.

(6) *Two similar Glass Cylinders and a Glass Rod.*—The former should hold rather more than 200cc. each, the point equivalent to that measure being marked on the glass.

(7) *A Burette*—marked to $\frac{1}{10}$cc. for the iron solution and an ordinary burette for the permanganate.

(8) *Three one cubic centimetre pipettes*—for the ferrocyanide, nitric acid, and sulphuric acid respectively, the one for the last being marked also to deliver $\frac{1}{2}$cc

The following is the method of analysis employed:—

A measured quantity of the water less than one litre in

bulk is taken, the amount being regulated according to the quantity of iron contained in the water, which is judged by a previously made qualitative experiment of adding 1cc. of the ferrocyanide to a portion of the oxidised water. One cubic centimetre of the sulphuric acid is added and then the permanganate from a burette till a permanent faint pink colour is obtained; the whole is made up to one litre, when it forms what may be called the "water test solution." Into each of the cylinders 1cc. of the potassium ferrocyanide is added and then a measured quantity of the water test solution put into one of them (x), both are next filled with water up to the mark and 1cc. of the standard nitric acid added to each. After (x) has been well stirred the standard iron solution is gradully run into (y), the liquid being stirred after each addition, and the colours in the two cylinders compared by placing them side by side over a sheet of white paper in front of a window; this is repeated till the colours in each of the cylinders appear to be equal, which point completes the operation.

Every cubic centimetre of iron solution used corresponds to 0·1 mgrm. of iron, from which the amount of iron added to cylinder (y) can be calculated. Then assuming that equal shades of colour are, *ceteris paribus*, produced by equal weights of iron, the amount of the latter in cylinder (x) is equal to that added to (y), and since the volume of the original sample of water in the test solution is known, and also the volume of the latter put into (x), the amount of iron in a measured quantity of water can therefore be calculated. The volume of the test solution put into cylinder (x) should be such as not to require more than 5cc. of the iron solution to be added to (y) to produce an equal shade, for if more be added the colour obtained would be too dark to compare with ease and accuracy.

If the sample of water contains such a small amount of iron as, after oxidation, not to give a coloration directly with the ferrocyanide and nitric acid, a sufficient quantity of it must be evaporated with ½ a cubic centimetre of the sulphuric acid till it occupies from 100 to 200cc. The

liquid is then poured into a flask, together with the rinsings, and oxidised with permanganate to a very slight excess, and then filtered so as to separate any precipitate, and also to destroy the excess of permanganate. The fluid thus obtained is next tested as before by adding the whole or a known part of it to one of the cylinders containing the ferrocyanide. When the water after being filtered has still a cloudy appearance, as is the case with sewage and polluted rivers, &c., a known quantity of the filtered water must be evaporated to dryness and ignited, the residue dissolved in a small quantity of hydrochloric acid and filtered, washed with water, and the free acid in the filtrate as nearly as possible neutralized with ammonia and then 1cc. of sulphuric acid, after which oxidised with permanganate, then filtered, if requisite, to destroy excess of permanganate, and the iron estimated as before. A green colour may be sometimes obtained instead of the pure blue; this is owing to a slight quantity of unreduced permanganate being present; this, however, is of no consequence, as with a little practice the green tint may be compared with the blue and correct results obtained; still the comparison may be rendered easier by adding 1 to 2 drops of permanganate to the cylinder to which the standard iron is run, and which by this means will also assume a green tint. Experiments were made with reference to this point and it was found that the presence of not more than a few drops of unreduced permanganate has little or no effect on the results obtained, the only consequence being the change of tint but not of depth of colour.

Potassium permanganate is employed as the oxidiser instead of the nitric acid, because (1) The oxidation is performed much quicker than it would be if nitric acid were used, and in the latter case the liquid would have to be heated. (2) It can be added to exactly the right point, which could not easily be done with nitric acid. (3) An excess of the latter is very detrimental to the accuracy of the method, for, from experiments made in relation to this point, it was found that when the amount of free acid pre-

sent in 200cc. was more than 0·0025cc. of the strong acid, it renders the colour deeper than it otherwise would be.

One cubic centimetre of the standard nitric acid is added to each of the cylinders, because (1) It renders the reaction much more delicate. (2) Because the colours produced in the presence of this amount of free acid are almost always of the same tint, being of a pure blue, whilst when no free acid is present the colour varies, even when apparently of the same depth, from a blue to a bluish green, which renders them less easy to compare. (3) Because it destroys the effect which the presence of a small quantity of any free acid, previously existing in the liquid, might have in altering the shade of colour produced, for from a series of experiments made with reference to this point also, it was found that when the amount of free acid present, in addition to the 1cc. of standard nitric acid added is only small, *i.e* less than 0·05cc. of the strong acid in 200cc. of water, it has no effect on the depth of colour produced. When any free acid exists in the water to be examined it must, before being oxidised, be made as nearly neutral as possible with ammonia, and the iron then determined.

The following are some of the results obtained on determining the iron in solutions of known strength :—

Iron Found. Milligrams.		Iron Calculated. Milligrams.
17·99	....	19·48
4·20	....	4·14
1·80	....	1·86
·61	......with salts (C) present......	·57
·51	....	·52
·40	....	·41
·40	... with KNO_3 (D) present ...	·40
·33	....	·30
·28	....	·31
·23	. ....with salts (B) present......	·22
·20	....	·21
·14	....	·16
·12	....	·10
·070	....	·078
·025	....	·031

In order also to test the effect which the presence of different salts has on this method, four series of experiments were made by adding known weights of the following salts to 1 litre of the ammonio-ferrous sulphate solution :

(A) Calcium sulphate, magnesium sulphate, ammonium chloride, sodium chloride, and potassium carbonate, in all 1·6 grm.

(B) Ditto, in all 0·9 grm.

(C) Magnesium sulphate, ammonium chloride, potassium carbonate, sodium chloride, and calcium chloride, in all 0·8 grm.

(D) Potassium nitrate 0·4 grm.

The solutions thus obtained were oxidised with permanganate and sulphuric acid diluted to one litre, and the iron estimated as previously described. The results obtained are given below, the letters attached denoting to which of the preceding series they severally belong, and from them it will be seen that the presence of these salts has little or no effect.

It was also found that neutral organic matter is not detrimental to the method.

With reference to the delicacy of the method it was found as a mean of seven experiments that 0·0055 mgrms. of iron give a very distinct colour on the surface, and that 0·015 mgrms. give a blue colour on being stirred with 200cc. of water, and therefore that 1 part of iron produces a blue coloration in 13,000,000 parts of water containing ferrocyanide of potassium and nitric acid. According to Hartig,* however, 1 part of iron (in the form of sulphate) only produces a colour in 600,000 parts of water containing ferrocyanide. The difference of these results is due to the effect which the presence of the small quantity of free nitric acid, added in the new method, has in increasing the delicacy of the reaction.

As to the smallest differences of reading which can be detected, it was found that when any quantity of iron solution below 1cc. had been added, a difference of 0·05cc. can be discriminated; above 1 and below 2cc. a difference of 0·1cc.; above 2 and below 4cc. a difference of 0·2cc.; and above 4 and below 5cc. a difference of 0·3cc.

* Jour. Pr. Chem. 22. 51.

The following are a few samples of different waters in which the iron has been determined as described above:

PARTS PER 1,000,000.

Date, 1874.	Name of Water.	Amount used in Analysis	By new method	By $KMnO_4$	As found by other Observers.
Feb. 14	Manchest. Water Supply	5 litres	·21		·287, R. A. Smith, 1864.
Mar. 20	,, ,,	1 ,,	·18		
——	,, ,,	2 ,,	·175		
July 15	,, ,,	1 ,,	·10		
——	,, ,,	1 ,,	·11		
Aug. 31	,, ,,	1 ,,	·27		
Sept. 1	,, ,,	1 ,,	·18		
,, 2	,, ,,	1 ,,	·26		
——	,, ,,	1 ,,	·27		
Sept. 3	,, ,,	½ ,,	·30		
——	,, ,,	1 ,,	·32		
Sept. 4	,, ,,	½ ,,	·38		
——	,, ,,	1 ,,	·36		
Sept. 5	,, ,,	½ ,,	·42		
——	,, ,,	1 ,,	·41		
Feb. 19	Medicinal Spring Trefriew, North Wales	30.cc	1600·00	1575·50*	
Feb. —	Chloride of Iron Spa, Harrogate	50.cc	290·00	290·34	289·40 H. Davies, Feb., 1872.
Feb. 16	R. Irwell nr. Pomona Gardens	¾ litre	·86		
Feb. 18	R. Mersey, Northenden	1½ ,,	·48		
Feb. 23	R. Dane, above Cong'ton	1 ,,	·57		
——	,, below ,,	¾ ,,	·44		
——	Liverpl. Water Works, Compensa. Water, White Cop'ce Heapy	2 ,,	·42		
Jan. 3	Barnsley Water Supply	2½ ,,	·25		
Feb. —	R. Thames, Lon. Bridge	1 ,,	·19		
Jan. 3	Cockerhm Well, Barnsl'y	2 ,,	·075		
Feb. —	New River Company ..	1 ,,	·050		Trace } Graham,
——	Lambeth ,, ..	1 ,,	·044		12·1(Al_2O_3 & Fe_2O_3) } Miller and
——	East London ,, ..	1 ,,	·038		6·8(,, ,,) } Hofmann.
Jan. 3	Friars' Well, Barnsley..	1¾ ,,	·005		

* The author hopes shortly to bring an analysis of this water before the Society.

ERRATA.

In the last number of the Proceedings, page 6, line 8, for "Odontogigram" read Odontogram.

Line 17, for "indentification" read identification.

Ordinary Meeting, November 3rd, 1874.

Rev. WM. GASKELL, M.A., Vice-President, in the Chair.

Mr. William Carleton Williams, F.C.S.; Mr. Harry Grimshaw, F.C.S.; and Mr. William E. A. Axon, M.R.S.L., F.S.I., were elected Ordinary Members of the Society.

"On the Corrosion of Leaden Hot-water Cisterns," by Professor H. E. ROSCOE, F.R.S., &c.

As the question of the occurrence of lead in town's water has been brought forward in the daily papers, I think it right (whilst stating my experience of many years' duration to be that the Manchester Corporation water when cold does not take up lead from the pipes under ordinary circumstances) to guard persons from using for drinking purposes water drawn from hot-water cisterns made of lead.

My friend, Mr. Melland, Surgeon, of Rusholme, handed to me a white powder taken from the inside of the covering of his leaden hot-water cistern, which presented a honeycombed surface, and in many places stalactitic masses hung down which were from $\frac{1}{4}$ to $\frac{1}{3}$ of an inch in length.

This powder consisted of a hydrocarbonate of lead, giving the following results on analysis :

Lead Oxide (PbO)	85·67
Carbonic Acid (CO_2)	12·12
Water (H_2O)	2·21
	100·00

It was doubtless formed by the solvent action of the condensed water containing oxygen upon the metal and the subsequent formation of the insoluble hydrocarbonate, and there can be little doubt that water drawn from such a cistern would be contaminated with lead.

"On an Improvement of the Bunsen Burner for Spectrum Analysis," by Mr. F. KINGDON, Assistant in the Physical Laboratory, Owens College.

The students in the Physical Laboratory of Owens College having occasionally experienced some difficulty in obtaining the spectra of some salts with the ordinary bunsen, through apparently a deficiency of pressure in the gas, it occurred to me that the amount of light even at this deficient temperature might be increased by multiplying the number of luminous points. This is accomplished by broadening out the flame of the bunsen, that is, causing the gas to issue through a narrow slit instead of a round hole. We have, so far, only made a rough experiment, the slit being about $\frac{7}{8}$in. long and $\frac{1}{8}$in. wide. The result is, as expected, a more brilliant spectrum.

"Some Notes on Pasigraphy," by HENRY H. HOWORTH, Esq., F.S.A.

Among the Utopian schemes which have interested others beside paradoxers and dreamers none has perhaps been more plausibly urged than the scheme of an universal language which should enable men to communicate with one another who are now inevitably sundered.

During the time of the Roman dominion it may have been hoped that Latin, which was its universal language, and in being so was also the language used by all those who in its point of view were not mere barbarians, would become in the future, as it was at the time, the universal language; be to the world what Hindustani is to the inhabitants of Northern India with their great variety of dialects, namely the common language of all. On the break up of the Roman Empire the Latin which was spoken so universally within its borders began to decay, and decayed differently in different localities, so that in a few centuries each fragment of the empire had developed a peculiar

tongue of its own, to a large extent unintelligible to its neighbours. Latin had then ceased to exist except as a dead and artificial language. It was no longer *the* universal language. But it still remained the universal language of divinity, of philosophy, and in fact of the narrow arcana of the sciences studied during mediæval times; it remained the learned language in which cultured people could and did communicate. At the Reformation this began to alter; culture ceased to be the peculiar heritage of the Romance peoples, and the so-called barbarians beyond the Rhine began to elbow their way into the arena of science, literature, and art, and gradually to acquire a peculiar vantage in all three. These barbarians spoke a language whose pedigree was not rooted in Latin. Latin to them was a foreign tongue, not as it was to Frenchmen and Italians, their grandmother-language, if I may coin the phrase. For a while its difficulties were eagerly mastered by the most patient race of students that ever lived; and like the rest of Europe for a while the Germans used Latin in their various learned compositions; but it was gradually disused. More and more valuable matter was published in the vernacular, and this was imitated rapidly elsewhere, so that the Latin language ceased to be a practical means of communication, and the Babel of tongues replaced the one uniform medium of intercourse. There can be no doubt that this was a serious loss. There are some people who value the mere knowledge of a language for its own sake, and acquire with equal delight and ease two or three languages. To others the difficulty is as great as is the grievance, and among the latter class are numbered many who value a language not for itself, who look upon the knowledge of several languages as a mere feat of memory in which one knows several names for each idea instead of one, and only value the knowledge of a language in so far as it gives them a key to the knowledge which is buried under it. This class

was no doubt aggrieved by having to learn French and German in order to be even with the latest discoveries. But to learn German and French was at least possible to a studious man. As time went on, however, other races besides the German and Romance speaking folk began to have a valuable literature. Russians, Bohemians, Hungarians, Scandinavians, *et id genus omne.* More lately still, a vast revival is on the point of breaking out again in the further east, and we may expect that Hindus and Chinese will be aspirants for at least a place in the great threshing mill where science and literature thresh their harvests. It is clearly appalling to give an outlook into the future and to contemplate the time when each man must be a Mezzofanti in order to prepare himself for literary or scientific work. Nor is this a mere chimæra. The Russians, who are very diligent students, are yearly giving up their old practice of composing in French and German, and will write only in their own difficult tongue; and perhaps I feel my toes especially pinched, inasmuch as they are working in mines where the ore I chiefly value is found, namely those relating to Eastern Ethnography. This frightful difficulty is naturally of interest to others besides dreamers, and it has often been suggested that all should agree once more to write in some language which, though artificial to most of its students, should yet enable all to communicate with one another. But this seems at present hopeless. English, from its wide extension, from the enormous number of individuals in Europe, America, and Australia who speak it, and from the general familiarity of most students with it, might promise such an end, but the Russians point to their future no less proudly than we to our present, and the mutual jealousies of nations make it impossible that any living language should be thus used, while the dead languages, perfect as they are for polish and rhetorical excellencies, are deficient both in plasticity and vocabulary when

it is attempted to employ them to meet the various requirements of our complicated scientific and other nomenclature.

Difference of language, although the greatest, is not the only bar to communication. Another lesser but still very appreciable difficulty is the variety of characters and alphabets in use in various parts of the world. Thus in Europe alone we have, beside the ordinary Italian characters, the German, Greek, and Cyrillic or Russian; and if we go further east, among those languages where the so-called tones prevail, or where gutturals of different kinds are in use, and where our limited alphabet becomes almost useless, we have a large number of peculiar alphabets, each one an irritating barrier to free intercourse with the language.

In the presence of these various difficulties it has been thought by some that a scheme might be devised by which communication might be carried on independently of language; that by a system of ideographs instead of words we might invent a conventional formulary, by means of which the common ideas of men might have a common representation; that we might do, in fact, what was done in the earliest period of writing, what the Chinese, the Mexicans, the Egyptians, and the Akkadians of Mesopotamia did, namely, use characters which should represent ideas and not words or sounds. It is well known that this is still the case in China; that many people can read Chinese books who know nothing or very little of the Chinese language. The Japanese, for instance, whose language is entirely different, are taught in their schools to read Chinese books without being taught the Chinese language. Such a system, if universally accepted, would be well described by the term pasigraphy. It is such a system which has been developed with great sacrifice and patience by a German savant called M. Bachmaier, and which I wish to bring before you to-night. Without expressing any opinion as to its utility, there can be no doubt as to its being quite practicable and easy to

learn; and my friend Dr. Birch, of the British Museum, has told me that he has used it with ease in correspondence.

The systems of picture writing above-named are all more or less cumbrous and difficult of application, because they employ an immense number of signs. Even the Chinese, which is the most satisfactory of these systems, and has by an ingenious system of combination reduced its simple forms to about 300, is very cumbrous. The system which I now introduce to you is much simpler, in that there are only 10 simple signs, namely the 10 first numbers, from the combination of which any vocabulary of any length can be represented, while it is an easy matter by a few simple marks to represent grammatical forms, and thus enable us to put down actual phrases and sentences, and not merely so many crude and substantive ideas which it requires an effort of the mind to combine into either sentences or logical propositions.

The principle of the plan is this:—A dictionary is prepared of each language: this may be of any length. In the scheme of M. Bachmaier 4,334 words have been chosen, by means of which an interchange of ideas on nearly any subject can be carried on. The words in these dictionaries each have a number attached to them, beginning with 1 and ending with 4,334. These numbers correspond to exactly the same words in each language; thus, if heart have the number 26 attached to it in English, 26 will be attached to cœur in the French, and to herz in the German dictionary, etc. etc. Each number thus becomes, therefore, an ideograph, and represents one idea to those who speak a variety of languages. Instead of communicating by words, therefore, we here have a simple, easy, conventionalism, by which we communicate by signs; a code of signals, in fact, which mean the same thing to all those who can read them.

These dictionaries, again, are twofold; in the one part we can find the word we need and its corresponding number.

This is the part used by the writer or the person making the communication. In the other part the numbers are ranged in order with their meanings attached, so that if one gets a communication consisting of a series of ideographic numbers, one has only to turn to this part of the dictionary to find a key to each. In corresponding, therefore, with a China man, 2 dictionaries would be needed, one an English dictionary would enable the person in England to find the ideographs answering to the words or ideas he wishes to communicate; the other a Chinese dictionary, in which these same ideographs could be read off into Chinese by a China man. This without the labour of acquiring a knowledge of Chinese characters, etc. etc.

So far we have rivalled the more ancient systems of ideography in completeness and have much simplified them. It next becomes clear that we may by very slight marks about our figures qualify the ideas so as to convey what is otherwise conveyed by the grammatical structure of language, by inflection, etc.

Dr. Bachmaier has drawn up a list of such signs which may be easily applied, thus; masculine or feminine are denoted by a slight mark above or below the first digit of the figure, as—$\bar{1}26$ masculine, $\underline{1}26$ feminine; the plural is denoted by a mark extending entirely beneath the number, as—$\underline{126}$; a substantive is denoted by an accent, thus—$\acute{1}26$; an adjective by another kind of accent, thus—$\hat{1}26$; a verb by a wavy line, as—$\widetilde{126}$; the past tense by a line through the figures 126, and the future by one above them, as—$\overline{126}$; the comparative and superlative degrees by dots, as—$1\dot{2}6$ comparative, $1\ddot{2}6$ superlative. Cases, by adding the numbers representing the prepositions that qualify the particular case, and so on. One or two rules to govern the position of the verb and of the adjective in the sentence and all is learnt that is required to enable us to communicate. The rapidity with which it can be done is a matter of practice,

and for purposes of correspondence, of telegraphy, and of short communications, it seems difficult to find an objection to its use. It has been tested, as I said, by Dr. Birch and others, and by them found to answer admirably, and if by its means the problem be eventually solved by which the vast gulf may be bridged which now separates those thinkers who do not know each other's language, a very great gain will have been secured. Its inventor, at least, is hopeful enough, and has, I believe, in preparation a series of dictionaries in several languages; of these I exhibit three which were distributed at the late Oriental Congress, viz., the English, French, and German ones.

"On the Existence of a Lunar Atmosphere," by DAVID WINSTANLEY, Esq.

The non-existence of a lunar atmosphere is spoken of by many astronomical writers as confidently as if it were a demonstrated fact. It is certain, however, that it is not a demonstrated fact, and it is certain also that if a fact at all it is undemonstrable.

The failure of any optical test, however delicate, to detect the existence of such an atmosphere still leaves another alternative open to us than the inference of atmospheric non-existence, namely, the existence of an atmosphere in quantity below the minimum discernible by the means employed.

The non-existence of an atmosphere about the moon comparable in density with that which surrounds the earth may indeed be regarded as an established fact; but the total non-existence of such an atmosphere is certainly an unwarranted conclusion.

I shall endeavour to show presently that the refraction of a ray of light is not the most delicate test which can be employed for the determination of the point in question. But even this test has yielded indications which astronomers of note have construed into evidence of a lunar atmosphere

of considerable attenuation. Arago, for instance, has observed on more than one occasion the apparent adhesion of a star for three or four seconds to the dark limb of the moon after it had been perceived to be in contact with it, and he has also observed a very sensible diminution of brightness previous to immersion.

From the distortion of the visible segment of the solar disc observed by Euler during the eclipse of 1748, Du Sejour, after making corrections for the effects of irradiation, arrived at the conclusion that the moon possesses an atmosphere having a horizontal refraction of 1.5″, and which is therefore 1,400 times more rare than common atmospheric air upon the surface of the earth.

A phenomenon similar in kind to the twilight of the earth has been recognised by Shroeter, in the form of a faint crepuscular light extending from each of the lunar cusps along the circumference of the unenlightened portion of her disc, from which he has been enabled to deduce the existence of an atmospheric envelope about our satellite capable at an elevation of 5,000 feet above her surface of causing a sensible inflexion of the light proceeding from a celestial body. But as the moon would describe an arc representing this amount in less than two seconds of time, "the circumstance has been adduced as affording a sufficient explanation of the difficulty of detecting a lunar atmosphere in the phenomena of occultations."

Chromatic dispersion is the test which in certain circumstances at any rate seems to offer better opportunities for ascertaining positively the existence of a lunar atmosphere than the test of simple refraction. It would cause the colour of an occulted object to change, making it green, and finally blue at the instant of disappearance.

The direct telescopic observation, by which alone this appearance could be noticed, would, from the operation of circumstances upon which I need not dwell, probably lead

only to results of uncertainty. At the same time it is but proper to remark that appearances have been observed at eclipses of the sun suggestive of this phenomenon, and which have been interpreted by Flamsteed as indicating the existence of a lunar atmosphere. The particular circumstances in which, as I take it, chromatic dispersion may afford weighty evidence of the existence of a lunar atmosphere occur when the body occulted by the moon is one of considerable angular magnitude and great intrinsic splendour. Then it is manifest that the chromatic dispersion effected by such an atmosphere would cause the projections of prismatic bands upon the earth, forming as it were an iris on the borders of the shadow, and bathing the landscape and the clouds in all the rainbow hues. These circumstances it will be seen exist during the totality of a solar eclipse, and the rainbow hues bathing alike the landscape and the sky, which I have indicated as the inevitable consequences of chromatic dispersion by a lunar atmosphere, would seem to be almost constant accompaniments of such eclipses. "As early as the year 840 it was remarked that during the total eclipse of the sun which happened in that year the colours of objects on the earth were changed." "Kepler mentions that during the eclipse which occurred in the Autumn of 1590 the reapers in Styria noticed that everything had a yellow tinge," whilst during that which took place in 1706 objects were observed to change their colour, now appearing of an orange yellow, and now of a reddish tinge.

The illustrious Edmund Halley remarked that the face and colour of the sky were changed during the eclipse observed by him in 1715. "The serene azure of the sky," he says, "turned to a more dusky livid colour, intermingled with a tinge of purple, and grew darker and darker until the total immersion of the sun." Sir John Clarke, in his account of the eclipse of 1737, states that "the ground was

covered with a dark greenish colour," whilst in the eclipse of 1842 "it was *universally* remarked that the colours of terrestrial objects were changed." Mr. Hind says that after the totality had commenced "the southern heavens were of a uniform and purple grey." "In the zenith and north of it the heavens were of a purplish violet, while in the north-west and north-east broad bands of yellowish crimson light, intensely bright, produced an effect which no person who witnessed it can forget." "The crimson," he says, appeared to run over large portions of the sky, *irrespective of the clouds,*" a circumstance certainly suggestive of a cause differing from that which gives rise to the hues of sunset." "All nature," continues Mr. Hind, "seemed to be overshadowed by an unnatural gloom; the distant hills were hardly visible; the sea turned lurid red, and persons standing near the observer had a pale and livid look." Not only did the colours "run over large portions of the sky, irrespective of the clouds," but they were visible at stations so remote from one another as to give additional assurance of an extra-terrestrial origin. The *distribution* of the colours observed by Mr. Hind at Ræelsberg is consistent with the theory of their production by the chromatic dispersion of a lunar atmosphere, whilst the *order of their succession,* as stated by Sig. Piola who observed in Italy, and the sudden *change* of colours noticed by Mr. Lowe, and which took place as the shadow swept along, afford confirmation of the theory.

It has been suggested by Mr. Lockyer that the colours projected upon the landscape during the continuance of a total solar eclipse may be those of various layers of the chromosphere alternately disclosed by that great screen the moon in its passage over the solar disc. Manifestly, however, the purity of some of the colours would be interfered with, assuming them to be produced in this manner, and the yellow which is so frequently seen would seem to be un-

accounted for. It is not unlikely that further and special observations will be required to say of either of these theories that it may or may not be maintained.

In the meantime, considering that the non-existence of a lunar atmosphere is undemonstrated and undemonstrable, that it is in opposition to analogy, and that even simple refraction has given evidence of such an inconsiderable atmospheric envelope as we might at most expect a body of the moon's small mass to have, it certainly seems to me that the balance of probability lies in favour of the theory that the rainbow hues observed at total eclipses of the sun are really the results of chromatic dispersion effected by a lunar atmosphere.

Ordinary Meeting, November 17th, 1874.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"Some Remarks on Dalton's First Table of Atomic Weights," by Professor HENRY E. ROSCOE, F.R.S.

As the Society is aware, the first table, containing the relative weights of the ultimate particles of gaseous and other bodies, was published as the 8th and last paragraph to a paper by Dalton, "On the Absorption of Gases by Water and other Liquids," read before this Society on October 21, 1803, but not printed until the year 1805. There appears reason to believe these numbers were obtained by Dalton after the date at which the paper was read, and that the paragraph in question was inserted at the time the paper was printed. The remarkable words with which he introduces this great principle give us but little clue to the methods which he employed for the determination of these first chemical constants, whilst in no subsequent publication, as in none of the papers which have come to light since his death, do we find any detailed explanation of how these actual numbers were arrived at. He says, * "I am nearly persuaded that the circumstance" (viz. that of the different solubilities of gases in water) "depends upon the weight and number of the ultimate particles of the several gases—those whose particles are lightest and single being less absorbable, and the others more, according as they increase in weight and complexity. An inquiry into the relative weights of the ultimate particles of bodies is a subject, so far as I know, entirely new. I have been lately prosecuting this enquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments."

* Manch. Mem., Vol. I., 2nd Series, p. 286.

Here follows the table of the relative weights of the atoms :—

TABLE

Of the relative weights of the ultimate particles of gaseous and other matters.

Hydrogen	1	Nitrous oxide	13·7
Azot	4·2	Sulphur	14·4
Carbone	4·3	Nitric acid	15·2
Ammonia	5·2	Sulphuretted hydrogen	15·4
Oxygen	5·5	Carbonic acid	15·3
Water	6·5	Alcohol	15·1
Phosphorus	7·2	Sulphureous acid	19·9
Phosphurretted hydrogen	8·2	Sulphuric acid	25·4
Nitrous gas	9·3	Carburetted hydrogen from stagnant water	6·3
Ether	9·6		
Gaseous oxide of carbone	9·8	Olefiant gas	5·3

In the 2nd part of his New System of Chemical Philosophy, published in 1810, Dalton points out under the description of each substance the experimental evidence upon which its composition is based, and explains, in some cases, how he arrived at the relative weights of the ultimate particles in question. Between the years 1805 and 1810, however, considerable changes had been made by Dalton in the numbers; the table found in the first part of the New System being not only much more extended, but in many cases the numbers differing altogether from those given in the first table published in 1805. It is therefore, unfortunately, to a considerable extent now a matter of conjecture how Dalton arrived at the first set of numbers. All we know is that it was mainly by the consideration of the composition of certain simple gaseous compounds of the elements that he arrived at his conclusions, and in order that we may form some idea of the data he employed we must make use of the knowledge which chemists at that time (1803–5) possessed concerning the composition of the more simple compound gases.

As I can find no record of any explanation of these early numbers I venture to bring the following attempt to trace their origin before the Society to whom we owe their first publication.

The first point to ascertain, if possible, is how Dalton

arrived at the relation between the atomic weights of hydrogen and oxygen given in the table as 1 to 5·5 (but altered to 7 in 1808). The composition of water by weight had been ascertained by the experiments of Cavendish and Lavoisier to be represented by the numbers 15 of hydrogen to 85 of oxygen, and the result was generally accepted by chemists at the time, amongst others doubtless by Dalton. That in those early days Dalton had actually repeated or confirmed these experiments appears improbable. At any rate he formed the opinion that water was what he called a binary compound, *i.e.*, that it is made up of one atom of oxygen and one atom of hydrogen combined together. Hence if he took the numbers 85 to 15 as giving the composition of water, the relation of Hydrogen=1 to Oxygen would be as 1 to 5·6, or nearly that which he adopted. It does not appear possible to explain why Dalton adopted 5·5 instead of 5·6 for oxygen; it may perhaps have been a mistake or a misprint, as there are two evident mistakes in the table, viz., 13·7 for nitrous oxide instead of 13·9, and 9·3 for nitrous gas instead of 9·7.

Let us next endeavour to ascertain how he obtained the number 4·3 for carbon (altered to 5 in 1808 and 5·4 later on). Lavoisier, in the autumn of 1783, had ascertained the composition of carbonic acid gas by heating a given weight of carbon with oxide of lead, and he came to the conclusion that the gas contained 28 parts by weight of carbon to 72 parts by weight of oxygen. Now Dalton was not only acquainted with the properties and composition of carbonic acid, but he was aware that Cruikshank had shown in 1800 that the only other known compound of carbon and oxygen, carbonic oxide gas, yields its own bulk of carbonic acid when mixed with oxygen and burnt; and also that Desormes* analysed both these gases, finding carbonic oxide to contain 44 of carbon to 56 of oxygen, whilst carbonic

* Ann de Chimie, T. 39, p. 38.

acid contained to 44 of carbon 112 of oxygen, being just double of that in the carbonic oxide. Dalton adds, "this most striking circumstance seems to have wholly escaped their notice." Hence Dalton assumed that one atom of carbon is united in the case of carbonic oxide with one atom of oxygen, whilst carbonic acid possessed the more complicated composition and contains two atoms of oxygen to one of carbon. Now if carbonic acid contains carbon and oxygen in the proportion of 28 to 72, carbonic oxide must contain half as much oxygen, viz., 28 of carbon to 36 of oxygen, and assuming that the atomic weight of oxygen is 5·5 that of carbon must be $\frac{28 \times 5{\cdot}5}{36} = 4{\cdot}3$. Having thus arrived at the number 4·3 as the first atomic weight of carbon, it is easy to see why Dalton gave 6·3 as the atomic weight of carburetted hydrogen from stagnant water, and 5·3 as that of olefiant gas. The one represents 1 atom of carbon to 2 of hydrogen, the other 1 of carbon to 1 of hydrogen, or olefiant gas contains two equal quantities of carbon, only half as much hydrogen as marsh gas. This conclusion doubtless expressed the results of Dalton's own experiments upon these two gases which were made, as we know from himself, in the year 1804. He proved that neither of these gases contains anything besides carbon and hydrogen, and ascertained—by exploding with oxygen in a Volta's Eudiometer—that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does, and that "just half of the oxygen expended on its combustion was applied to the hydrogen and the other half to the charcoal. This leading fact afforded a clue to its constitution." Whereas, in the case of olefiant gas, two parts of oxygen are spent upon the charcoal and one part upon the hydrogen.

The atomic weight of nitrogen (azote = 4·2) was doubtless obtained from the consideration of the composition of ammonia, whose atomic weight is given in the table at 5·2.

Ammonia was discovered in 1774 by Priestley, but the composition was ascertained by Berthollet in 1775, by splitting it into its constituent elements by means of electricity, when he came to the conclusion that it contained 0·193 parts by weight of hydrogen to 0·807 parts by weight of nitrogen. Dalton assumed that this substance is a compound of one atom of hydrogen with one of nitrogen, and hence he obtained for the atomic weight of azote $\frac{807 \times 1}{193} = 4{\cdot}2$; and $4{\cdot}2 + 1 = 5{\cdot}2$ as the atomic weight of ammonia. It is also probable that Dalton made use of the composition of the oxides of nitrogen for the purpose of obtaining the atomic weight of nitrogen. If we take the numbers obtained partly by Davy and partly by himself, as given on page 318 of the New System, as representing the composition of the three lowest oxides, it appears that the mean value for nitrogen is 4·3 when oxygen is taken as 5·5. In all probability the number in this table (4·2) was obtained from an experiment of Dalton's made at an earlier date.

It is not possible to ascertain the exact grounds upon which Dalton gave the number 7·2 for phosphorus; its juxtaposition, however, in the table to phosphuretted hydrogen shows that it was probably an analysis or a density determination of this gas which led him to the atomic weight 7·2, under the supposition that this gas (like ammonia) consisted of one atom of each of its components. In the second table, published in 1808, Dalton gives the number 9 as that of the relative weight of the phosphorus atom, and we are able to trace the origin of this latter number, although that of 7·2 is lost to us. On p. 460, Part II. of his New System, Dalton states that he found 100 cubic inches of phosphuretted hydrogen to weigh 26 grains, the same bulk of hydrogen weighing 2·5 grains; hence, assuming that equal volumes contain an equal number of atoms, we have: $\frac{26 - 2{\cdot}5}{2{\cdot}5} = 9{\cdot}4$ gives the atomic weight of phosphorus

nearly. It was probably by similar reasoning from a still more inaccurate experiment than this one that he obtained the number 7·2.

Sulphur, which stands in the first table of 1803 at 14·4, was altered in the list published in the New System to 13. These numbers were derived from a consideration (1) of the composition of sulphuretted hydrogen, which he regarded as a compound of one atom of sulphur with one of hydrogen, and (2) of that of sulphurous acid, which he supposed to contain one atom of sulphur to two of oxygen. Dalton knew that the first of these compounds contained its own volume of hydrogen, and he determined its specific gravity, so that by deducting from the weight of one volume of the gas that of one volume of hydrogen he would obtain the weight of the atom of sulphur compared to hydrogen as the unit. The specific gravity he obtained was about 1·23 (corresponding nearly he says—p. 451—to Thenard's number 1·23); hence (as he believed air to be 12 times as heavy as hydrogen) he would obtain the atomic weight of sulphur as $(12 \times 1{\cdot}23) - 1 = 13{\cdot}76$, which number, standing half way between 14·4 as given in the first table and 13 as given in the second, points out the origin of the first relative weight of the ultimate particle of sulphur. So from sulphurous acid he would obtain a similar number, taking the specific gravity as obtained by him (Part II. 389) to be 2·3, and remembering that this gas contains its own bulk of oxygen (p. 391), he obtained $(2{\cdot}3 - 1{\cdot}12) \times 12 = 14{\cdot}16$ for the atomic weight of sulphur. As however we do not possess the exact numbers of his specific gravity determinations, and as we do not exactly know what number he took at the time as representing the relations between the densities of air and hydrogen (in 1803 he says that the relation of $1 : 0{\cdot}077$ is not correct, and that $\frac{1}{20}$ is nearer the truth), it is impossible to obtain the exact numbers for sulphur as given in the first table.

In reviewing the experimental basis upon which Dalton

founded his conclusions, we cannot but be struck with the clearness of perception of truth which enabled him to argue correctly from inexact experiments. In the notable case, indeed, in which Dalton announces the first instance of combination in multiple proportion (Manch. Mem., vol. 1, series 2, page 250) the whole conclusion is based upon an erroneous experimental basis. If we repeat the experiment as described by Dalton we do not obtain the results he arrived at. Oxygen cannot as a fact be made to combine with nitric oxide in the proportions of one to two by merely varying the shape of the containing vessel, although by other means we can now effect these two acts of combination. We see, therefore, that Dalton's conclusions were correct, although in this case it appears to have been a mere chance that his experimental results rendered such a conclusion possible.

"Action of Light on certain Vanadium Compounds," by Mr. JAMES GIBBONS. Communicated by Professor H. E. ROSCOE, F.R.S.

Potassium divanadate, in combination with organic matter, is first rendered green, and ultimately blue, by exposure to light, being reduced probably to the state of vanadium tetroxide. The salt is not sensitive to light in the absence of organic matter.

Gelatine, mixed with potassium divanadate, becomes slightly less soluble in warm water after being exposed to light; this is apparent by the unexposed portions of the film swelling and dissolving more quickly when treated with water than the exposed parts.

If a colourless film of dry sodium orthovanadate (Na_3VO_4), free from organic matter, be exposed on glass to the sun for several hours, it only acquires a faint brown tint. The film kept in the dark, with access of air for some hours, regains its normal colourless condition. The salt does not undergo any change when exposed to diffused daylight.

Paper, which does not contain any size of an animal origin, when coated with a solution of sodium orthovanadate, is darkened on exposure to light, the depth of tint depending on the length of exposure and on the strength of the solution used. The tint, however, never becomes darker than a slate colour.

If the paper thus prepared be immersed, after exposure to light, in a solution of silver nitrate, the colour in the exposed part instantly changes to a deep brown or to a black colour, varying according to the amount of exposure. A tint of the decomposed vanadate, which is of so slight an amount as to be with difficulty distinguished from the whiteness of the paper, will, by immersion in the silver nitrate, be toned so as to exhibit a very perceptible tint.

It is evident that paper prepared in this way might be employed for the purposes of photographic printing.

The unexposed parts are converted, by treatment in the silver bath, into yellow silver vanadate. This substance may be dissolved out either by ammonia or by sodium hyposulphite. This act of fixing converts the dark brown or black part into those of a red colour. This may be prevented to some extent by using a bath of ammonio-silver nitrate, with an excess of ammonia, instead of the simple silver nitrate bath. The developed print can afterwards be toned with gold chloride.

The length of exposure required to produce a deep black is about one hour to a strong sun light. This by using a solution of the sodium orthovanadate containing about 11 per cent of the salt.

Some ligneous substance only must be present with the sodium orthovanadate for the production of the above-mentioned slaty tint; for if an albuminous body be present, a faint brown tint is produced after exposure to light, and the silver nitrate is not afterwards reduced to any very great extent. The slate colour of the reduced salt appears to be due to the formation of vanadium trioxide. If the exposed

paper be kept for some weeks its colour changes to that of a yellowish brown, free vanadic acid appearing to be produced.

Gelatine impregnated with sodium orthovanadate exposed to light, and afterwards dipped into a solution of silver nitrate, becomes insoluble in hot water.

Silver orthovanadate is capable of forming a photographic image, which is nearly latent, and which may be developed by the ordinary ferrous developer used in photography.

To produce this image two or three minutes' exposure to sunlight is required. To develope it, it is essential that little or no silver nitrate be present; otherwise, the exposed and unexposed parts are reduced indiscriminately. The washed silver vanadate can be mixed with a solution of gelatine containing a little albumen, spread upon paper, and allowed to dry; it can then be exposed to light, and afterwards developed.

"On Basic Calcium Chloride," by HARRY GRIMSHAW, F.C.S.

When a strong solution of calcium chloride is boiled with calcium hydrate, the solution filtered, and allowed to cool, a salt separates out in long, slender, needle-shaped crystals. This salt is called in Gmelin's Handbook, hydrated chloride of calcium and lime, or hydrated tetra-hydrochlorate of lime, and, according to him, has been noticed by Bucholtz and Trommsdorf and by Berthollet, and analysed by H. Rose, who found the formula $3CaO, CaCl+16Aq$, or in present notation $3CaO, CaCl_2+16H_2O$. This is not a very simple or intelligible formula, and moreover the percentage of water found, which was 49·084, is considerably lower than that required by the formula, namely, 50·793. The salt was therefore prepared and analysed as follows.

The solution of calcium chloride was prepared by dissolving, by heat, pure white marble in moderately concentrated hydrochloric acid until saturated. This was boiled with an

excess of milk of lime for about an hour, filtered whilst hot and allowed to cool. The salt separated out, on standing, in slender, white, needle-shaped crystals, generally from one half to an inch in length. They were sometimes obtained of not more than a quarter of an inch in length, and almost transparent, the separation not taking place until the solution was agitated. The composition of the crystals was in all cases the same, and was not affected by the length of time they were allowed to remain in contact with the liquid. The crystals, dried as quickly as possible between blotting paper, on analysis gave the following results:—

(*a*) 0·677 grm. gave 0·273 grm. CaO.
0·442 „ „ 0·228 „ AgCl and 0·0012 Ag.
0·307 grm. lost on heating 0·152 grm.

(*b*) 0·359 grm. gave 0·144 grm. CaO.
0·794 „ „ 0·2045 „ AgCl and 0·0245 Ag.
1·052 lost on heating 0·52 grm.

	Calculated for $CaO, CaOHCl + 7H_2O$.	Found (*a*)	Found (*b*)	Calculated for $3CaO, CaCl_2 + 16H_2O$.
Ca	29·144	28·80	28·77	28·22
Cl	12·932	12·86	12·794	12·522
O	5·829	—	—	8·465
OH	6·193	—	—	—
H_2O	45·901	—	—	—
Loss on heating	49·179	49·40	49·47	(H_2O) 50·793
	100·000			100·000

The constitution of the salt is therefore expressed by the formula $O\left\{\begin{matrix}CaOH\\CaCl\end{matrix}\right. + 7H_2O$. On heating, two molecules of the salt decompose, forming $3CaO + CaCl_2 + 15H_2O$. The difference of the proportion of the constituents according to the above formula, and according to that assigned by Rose, is therefore that corresponding to one atom of water more or less. As the amount of water is nearly 50 per cent of the whole, this constituent will exhibit the greatest difference, namely 1·514. Accordingly, the numbers above will

be found to agree with the formula $O \begin{cases} CaOH \\ CaCl \end{cases} + 7H_2O$, which loses on heating 49·179 per cent. The number found by Rose (49·084) agrees very nearly.

The salt is perfectly stable for any length of time if kept out of contact with the air. It may be also kept unaltered in the mother liquor for some time. In the air it decomposes, absorbing carbonic acid and water. Over sulphuric acid in vacuo or in air, or over quicklime, it parts with a portion of its water of crystallization. Both these circumstances interfere with the exact drying of the salt.

With water, it decomposes into calcium hydrate and calcium chloride—

$$2\left[O \begin{cases} CaOH \\ CaCl \end{cases}\right] + 2H_2O = 3Ca(OH)_2 + CaCl_2.$$

By the substitution of hydrobromic for the hydrochloric acid in the preparation of the salt, I expect to obtain a corresponding bromine compound.

"On the Structure of Stigmaria," by Professor W. C. Williamson, F.R.S.

At the meeting of the Manchester Literary and Philosophical Society, held on October 20th, Mr. Binney called in question some conclusions at which I had arrived, and had published in Part II. of my Memoirs on the Structure of the Coal Plants, respecting the organisation of Stigmaria. Mr. Binney further published an abstract of his remarks in Part II. of Vol. 14 of the Society's Proceedings. Believing that Mr. Binney's observations, if allowed to pass unnoticed, may mislead some Palæontologists unacquainted with Stigmaria, I feel called upon to reply to them through the same channel as that which he has employed for their promulgation. The general features of the plant, known for half a century as Stigmaria ficoides, have been so well described by Lindley and Hutton, Dr. Hooker, Mr. Binney, and Brongniart, that no one familiar with those descriptions can fail to recognise it without difficulty. That plant consisted of a

central medulla, surrounded by a cylinder of scalariform vessels arranged in radiating wedges, very distinctly separated by two kinds of medullary rays (primary and secondary), the whole being enclosed in a thick bark from the surface of which spring numerous large cylindrical rootlets. The vascular cylinder gives off numerous large vascular bundles of scalariform vessels, which proceed outwards, through the conspicuous primary medullary rays, to reach the rootlets.

The dispute between Mr. Binney and myself resolves itself chiefly into three points. 1st.—The structure of the medulla of Stigmaria. 2nd.—The source whence the vascular bundles supplying the rootlets are derived. And 3rd.—The nature of some vascular bundles which both Mr. Binney and M. Goeppert have figured as existing within the medulla, and one of which is prolonged radially in M. Goeppert's example through a medullary ray. Mr. Binney and M. Goeppert believe that the cellular medulla of Stigmaria contained bundles of very large scalariform vessels, and that those bundles proceeded outwards to supply the rootlets. On the other hand, in my 2nd Memoir, referred to by Mr. Binney, I not only expressed my conviction, but demonstrated the absolute certainty that such was not their origin. I adhere to the same opinion as I previously expressed, and have the specimens on the table which prove its correctness. The fact that these bundles were derived, not from the medulla, but from the vascular wedges of the woody cylinder, was illustrated by the figures 43, 44, and 47 of the Memoir referred to, figures which accurately represent, not conditions occasionally met with, but, those which characterise every specimen of the true Stigmaria ficoides. In the Memoir I further affirm that immediately within the woody cylinder there exists a delicate cellular tissue, and state that one of my specimens makes it perfectly clear that the entire medulla consisted of similar cells, unmixed with any vascular bundles whatever such as were represented in M. Goeppert's and Mr. Binney's figures, and the accuracy of which

was, and it appears still is, endorsed by Mr. Binney. After thus endorsing what I believe to be a grave mistake, Mr. Binney proceeds to justify his doing so by appealing to a specimen which I have not seen, but which Mr. Binney's own description convinces me is a plant altogether different, alike from THE Stigmaria of authors, and from M. Goeppert's and Mr. Binney's own figures. Mr. Binney describes his new specimen as having a radiating woody cylinder, immediately within which is a second series of large vessels not arranged in radiating wedges, and which Mr. Binney says is "something like a medullary sheath, enclosing a medulla composed of very small and short barred tubes or utricles, in which are mingled large vascular tubes or utricles." Though this use of vague terms renders the sense obscure, I presume that Mr. Binney simply means that in the medulla of his plant a *vascular* cylinder encloses a *cellular* medulla, or, in other words, that his specimen has a Diploxyloid axis. That Mr. Binney possesses a specimen having the above structure, and giving off rootlets from its periphery, I have no reason for doubting, since in the Memoirs already quoted I have described a similar structure under the name of Diploxylon Stigmarioideum, and respecting which I make the following observations:—"It is possible that the plant may, like Stigmaria, prove to be the uppermost part of a root of some of the other forms" (*i.e.* of Lepidodendroid stems), "though I have never yet found it associated with any rootlets, and it may be a fragment from the base where stem and roots united."—(loc. cit. p. 239.) I arrived at the above conclusions because I found in the specimen described evidence that large rootlet bundles were given off from the woody zone as in the true Stigmaria. But I affirm that out of hundreds of Stigmarian fragments that I have examined, I have only found two possessing this structure; and I unhesitatingly express my conviction that Mr. Binney's specimen is another example of an equally rare type, both being entirely distinct from Stigmaria ficoides,

to which latter plant alone is referable Mr. Binney's previously published figures, M. Goeppert's description and figures of which Mr. Binney approves, and mine which he rejects.

Mr. Binney proceeds to say, "The size of these large vascular tubes or utricles in the medulla, exceeding anything, so far as his knowledge extended, hitherto observed in fossil plants, shows that it was easily decomposed, and thus accounts for the general absence of the medulla in Sigillaria and its roots." At this reasoning I must altogether demur. Size has nothing whatever to do with the preservation of the tissues in fossil plants. Vascular structures strengthened by transverse bars of lignine are equally well preserved whether they are large or small. The medulla of Stigmaria disappeared or became much disorganised because it consisted of an unusually delicate cellular tissue with extremely thin walls. This tendency to decay was more manifest towards the centre of the medulla than at its circumference. Specimens on the table exhibit this peripheral part of the cellular medulla in exquisite perfection, giving off its characteristic cellular prolongations constituting the medullary rays, as described in my memoir. And yet this beautiful cellular tissue occupies the position which Mr. Binney says was occupied by "large vascular tubes or utricles." The specimens referred to showing these conditions constitute unanswerable facts.

Mr. Binney correctly notes the resemblance of the inner vascular cylinder in his specimen to a "medullary sheath." I have already said the same thing in several of my memoirs, and M. Brongniart said it before either of us. But this very homology, if correct, indicates the probability of Mr. Binney's specimen being a fragment derived from the junction of stem and root rather than a true root, since in living plants possessing a medullary sheath, that sheath, as every botanist knows, is never prolonged into the true roots, for the simple physiological reason that its origin

is directly connected with that of the leaf formations of the ascending axis.

As I have already observed, M. Goeppert's and Mr. Binney's previous figures represent a structure altogether different from that now described by Mr. Binney. Instead of the continuous inner vascular cylinder of the latter M. Goeppert's figure displays two detached, unsymmetrically arranged, vascular bundles in the interior of the medullary cavity. I have already affirmed my conviction that these belong to intruded rootlets of a Stigmaria, and are in no respects part of the true medullary axis. On the other hand Mr. Binney says that "they are certainly not intruded rootlets, as anyone who examines the learned author's plates can satisfy himself." On this point Mr. Carruthers writes to me on November 2nd, "No one who is accustomed to sections of Stigmaria can fail to see that Goeppert has mistaken the accidental rootlets of Stigmaria penetrating the decayed axis for an organic part of that axis." I may allow this opinion of an experienced botanist, with which I wholly concur, to neutralise that of Mr. Binney, who further says, "It is very improbable that they" (*i.e.* Goeppert's vascular rootlets) "had ever been introduced into the axis after the pith had been removed." To this I reply that it is an extremely rare thing to find any such axis which does not contain more or less of these rootlets. My cabinet is full of such examples, and in two specimens on the table, one of which has been lent me by Captain J. Aitken of Bacup, similar rootlets not only exist in the central axis but have penetrated the medullary rays as in M. Goeppert's specimen.

Mr. Binney, referring to my comments upon his previous memoir, says that "in that memoir mention is only made of the large vascular bundles found in the axis, without calling them vascular or any other vessels." I do not very clearly understand what this sentence means, but I presume it is intended to imply that Mr. Binney never affirmed that

the pith of Stigmaria contained VASCULAR tissues, and that I have misrepresented him in stating that he had done so. I can only answer this by giving Mr. Binney's words. "The most important circumstance thus developed is the existence of a double system of vessels in Stigmaria, first shown by Goeppert, and the consequent approach in this respect to Diploxylon, Corda. In Diploxylon, however, the inner system forms a continuous cylinder, concentric with and in juxtaposition to the wedges of wood forming the outer; while in Stigmaria the same inner system is broken up into scattered bundles, apparently unsymmetrically arranged in the medullary axis or pith of the plant"—*Quarterly Journal of the Geological Society*, vol. 15, p. 17—and on p. 78 of the same memoir, describing the specimen represented by fig. 2, he says, "The axis is filled with eleven or twelve large vessels of circular or oval form," and the same structures are again spoken of as "vessels" no less than six times in the next seventeen lines, with the further remark that "altogether these angular vessels remind me somewhat of the vascular tissue in the middle of Anabathra"—(loc. cit. p. 78). It is true that in two places Mr. Binney applies to these structures the term "utricles," by which, I presume, he means cells, but such a term, applied to such tissues, is equally applicable to all known fibro-vascular structures, and is simply equivalent to saying that scalariform vessels have no existence.

I have entered into these details because by promulgating vague and groundless doubts respecting work already carefully done, Mr. Binney's communication tends to reintroduce confusion into questions that have been virtually settled. It does this through failing to discriminate between things that differ. His introductory remarks refer to the common Stigmaria ficoides, whilst his justification of those remarks rests upon a plant of a very different character, and which I am absolutely certain is not the common form of Stigmaria.

Ordinary Meeting, December 1st, 1874.

Rev. WM. GASKELL, M.A., Vice-President, in the Chair.

"Some Doubts in regard to the Law of the Diffusion of Gases," by HENRY H. HOWORTH, Esq.

The Author said that he had a difficulty in reconciling the conclusions drawn by Dalton, Berthollet, and Graham respecting the diffusion of gases with the actual facts of nature, and it seemed to him that the only way in which the inferences drawn from experiments in the laboratory and what was going on in nature on a large scale could be reconciled was in the belief that either diffusion was extremely slow in some cases or that it was sometimes prevented.

He argued that the continuity of condition between gases and liquids which had lately been so admirably illustrated made it, *a priori*, probable that similar laws prevailed in both classes of matter in respect to their laws of diffusion, &c., and that, as in the case of liquids, the law of diffusion was not a universal one, but had at least apparent exceptions, so also among gases there might occasionally be conditions which resisted or very greatly impeded the operation of the law. He granted at once that the gases that form air, and many others, bear uniform testimony to the correctness of the generalisation; but in the case of carbonic acid, watery vapour, and hydrogen, there seemed to be some room for doubt. The fact that in all abandoned wells, tunnels, and mines, where atmospheric currents do not play, and where there is no absorbing vegetation, there is an accumulation of carbonic acid gas, although these hollows have ample access to the air, goes to show that the rate of diffusion must be exceedingly slow in these cases, if in fact diffusion be not actually suspended. The mephitic vapours

in low valleys in volcanic districts, and the gases that generate malaria in many low-lying marshy districts—such as the Maremma in Italy, the neighbourhood of Montpelier in France, &c.—seem to point in the same direction. The very diverse condition in regard to the amount of watery vapour contained in contiguous areas of the atmosphere, whether we examine it in different neighbouring localities or in the superimposed strata of air in any particular locality, show that the working of the law of diffusion is here greatly impeded. In regard to hydrogen the evidence is somewhat different. It is clear that if under certain conditions hydrogen be an exception to the general law of the diffusion of gases, and follows rather the more general law of gravitation, that it will exist in a stratum above the atmosphere and beyond the reach of direct observation. In his experiment upon the occlusion of gases, Mr. Graham examined several aerolites, and found that under the air pump they parted with a very large quantity of occluded hydrogen. If, as is probable, the gas was occluded by the aerolites when at a red heat, and this red heat was coincident with their passage through that layer of the upper atmosphere in which the phenomena of shooting stars and of the aurora occur, it seems more than probable that this stratum is a layer of hydrogen. This is confirmed by what we know of the spectrum of certain auroras, which resembles those of the zodiacal light and the solar corona. The spectrum of the corona has been the most attentively studied, and Jansen, perhaps the greatest authority on it, speaks most confidently about its distinguishing feature being the hydrogen lines, while a special line which characterises both its spectrum and that of the aurora, and which is different to that of any terrestrial substance, is considered by Father Secchi to be an abnormal hydrogen line. Dr. Dalton long ago argued, as Mr. Baxendell has reminded Mr. Howorth, that the peculiar features of the aurora could best

be explained by the hypothecation of a stratum of some peculiar gas above the atmosphere. A gas of a "ferruginous nature" is the expression of Dr. Dalton. Now hydrogen in the higher chemistry is not only classed among the metals, but Faraday and others have shown that in its relation to magnetism it is nearly allied to iron, so that a stratum of hydrogen above the air would seem to exactly answer Dr. Dalton's postulate. If it should exist, the earth would resemble the sun in one remarkable feature, for we now know that the sun is girdled with an immense layer of hydrogen. Lastly, he would add that the heterogeneous texture of the gaseous nebula, like the great nebula in Orion, seems to argue that the law of the equal diffusion of gases does not prevail there.

Mr. Howorth presented these facts with considerable hesitation, his excuse for doing so being his view that all physical laws are tentative only; that is, they are good as long as they explain all the facts, and no longer, and that it sometimes becomes a duty to present apparently aberrant and abnormal facts to an audience so well qualified to criticise them as the Manchester Literary and Philosophical Society, in order that they may either be brought within the law or that the law may be revised.

Ordinary Meeting, December 15th, 1874.

EDWARD SCHUNCK, PH.D., F.R.S., &c., President, in the Chair.

Mr. Joseph Carrick and Professor Morrison Watson, M.D., were elected ordinary members of the Society.

Rev. WM. GASKELL, M.A., read an interesting account of Horrocks' and Crabtree's Observations of the Transit of Venus in 1639, published in the Annual Register for 1769.

"Some Particulars respecting the Negro of the Neighbourhood of the Congo, West Africa," by WATSON SMITH, F.C.S.

These particulars were furnished by Mr. Richard C. Phillips in two letters dated respectively July 17th, 1873, and July 17, 1874. The parts of the coast are those situated between the towns Chillunga, Landana, Cabenda, Ambrizette, Kinsembo, and Ambriz.

With respect to the Coast trading, which chiefly consists of a system of barter, the following information is given. The articles of barter are chiefly rum, beads, cloth, knives, rings, hatchets, and miscellaneous articles of the kind. On certain parts of the coast some articles will pass as currency to the exclusion of others; thus beads are essential in Ambrizette and Kinsembo, being the true money of the country, and in consequence comparatively little spirits are used in those places, while at Loango rum plays a most prominent part. The produce of the country exchanged by the Negroes for the above mentioned articles is as follows: Palm nuts, ivory, coffee. No doubt numerous other articles are bartered, but those named are principal.

The African uses most of his rum as money. Suppose a Negro trader receives half a gallon; he will divide this into many portions—drinking some, giving some to his wives (perhaps ten), spending some in palm nuts, casada, corn, firewood, &c. The rum is preferred, partly on account of its being readily divided without suffering loss of value. The same applies to beads, which are preferred in some localities. The drinking of spirits is thus very much limited by its scarcity, and by its use as currency; yet occasionally drunkenness breaks out and a "big dance" is held. One thing is evident, viz., that a settled and confirmed taste for spirits is being formed amongst these coast natives. Also, that this being a tropical country, this fact points to a great peril in store for the future of the native people; and not a

very vivid imagination might picture the effect probably following the introduction in larger quantity of such a beverage, or say the method of manufacturing the article for themselves.

The character of the Negro is thus described by Mr. Phillips: "He is very averse to work, and takes little thought for the future, has little love or hate, is not revengeful, as that would entail trouble or expense, lives unto himself alone. Crafty, cunning, a born swindler, often a confessed rogue, avaricious yet lazy, he generally attaches himself to some one of importance and does his bidding like a stray cur who follows you home, and of which you take charge. This is not the individual, but the national character." In the early part of this year, 1874, the small-pox broke out violently on the Coast. Mr. Phillips at once set to work to grapple with the evil in a manner which is beyond praise. Having procured a quantity of vaccine lymph, he vaccinated some hundreds of the Negroes. The result exceeded his expectations, for not one died who had been vaccinated, though numbers fell around them. Later on, Mr. Phillips writes, "I am entirely out of lymph, and, in connection with this, I'll tell you something. When I had finished the 'batch' of which I have spoken I told several to come at the time of ripening, so that I could transfer the matter for the use of others; but not one had the generosity to remember his neighbours, and not one came." Mr. Phillips further writes: "I believe if the vaccine lymph be fresh, be it syphilitic, cancerous, scrofulous, what not, it acts in precisely the same manner."

The social customs are of a very peculiar nature, and certain of them furnish, as I have recently found, an interesting contribution to the elucidation of a question connected with ancient Greek History. Mr. Phillips writes: "The women are the slaves of their husbands or the mother's eldest brother. This may seem a strange arrange-

ment. It is after this principle—If I die leaving children they may not be mine really, but another man's; but my eldest sister is certainly of my blood, having been born of the same mother, then her eldest son is certainly of her, consequently of my blood. My property would then go to her children, not my wife's and (presumed) mine. In like manner I should have the control of my sister's children during my life. My sister's husband cannot take the charge of his children because they cannot be proved to be his; but on my side there can be no mistake, they are of my blood. This arrangement is necessary on account of concubinage, which is every woman's portion until she is married, and illegitimacy is a term of which they have no idea. One and all children are alike, the mother's eldest brother having charge of the whole. They seem to get on very well without the domestic strife which one would expect; the wives seem contented and happy. In fact, before the manner in which the domestic machinery works can be appreciated, an examination of it is necessary. Now in Dr. Ernst Curtius' History of Greece, translated by Prof. Ward, of the Owens College, the following paragraph occurs (p. 83) concerning the Lycians, a people of Greek descent, who lived in Lycia, a country of Asia Minor. Quoting the opinions of that day, Dr. Curtius writes: "Their patriotism they proved in heroic struggles, and in the quiet of home developed a greater refinement of manners, to which the special honour in which they held the female sex bears marked testimony." "This is one of the blessings of the religion of Apollo," &c., &c. "*And in the families of the citizens the matrons were honoured by the sons designating their descent by the names of their mothers.*"

In a foot-note Dr. Curtius explains as follows: "It is true that the usage of the Lycians to designate descent by the mother was interpreted even in ancient times as a proof that in their social life they conceded a peculiar influence to

women. They are called women's servants by Heraclides Ponticus. However, it would be an error to understand the usage in question as a homage offered to the female sex. It is rather rooted in primitive conditions of society, in which monogamy was not yet established with sufficient certainty to enable descent on the father's side to be affirmed with assurance. Accordingly this usage extends far beyond the territory commanded by the Lycian nationality. It occurs even to this day in India. It may be demonstrated to have existed amongst the ancient Egyptians. It is mentioned by Sanchuniathon, where the reason for its existence is stated with great freedom. Hence we must regard the employment of the maternal name for the designation of descent as the remains of an imperfect condition of social life and family law, which as life became more regulated was relinquished in favour of the usage, afterwards universal in Greece, of naming children after the father. This diversity of usage, which is of extreme importance for the history of ancient civilisation, has been recently discussed by Bachofen."

With respect to the language, Mr. Phillips furnishes me with the following interesting particulars:—"The native tongue is very peculiar, but very musical. I fancy it would sound well sung. The sound of one word often determines that of several others in a sentence, as lĭ-īlu-lĕ ămi, chinkutuchi-ami, malo-mami, where the alliteration is plainly perceived. Ami means 'my.'" In another letter is the following: "The language is a study of great interest, being one of the Bantu or alliterative languages, the peculiarity of which consists of euphonious changes of consonants for the sake of the alliteration. Thus one sound will often predominate through a sentence governed by some principal word. It may be considered in the following light: Suppose it to contain several nouns—five or six—and each gender to depend on the sound of the first syllable, then let the pronouns, adjectives, verbs, all be declined, with the

same corresponding generic sounds; thus we get an alliteration, the subject governing all until the object is reached. The inflexions are of the first, not the last syllable. The following are a few specimens of singular and plural with personal pronouns attached, illustrating the theory, which however is not invariably carried out:—

Chinkŭtŭ chiămi *shirt my*	my shirt.	Lŭngŏ chiami *ring my*	my ring. (*No alliteration*)
Binkutu biami *shirts my*	my shirts.	Lungŏ biamī *rings my*	my rings. (*No alliteration*)
Li-ĭlu leami *nose my*	my nose.	Mwŏnŏ ami *child my*	my child.
Mātŭ māmi *eyes my*	my eyes.	Bānŏ bami *children my*	my children.

There are some very interesting forms of verbs, adverbs of negation, &c., phrases, for example—toto twāmĭ
sleep my I am asleep, or was asleep at the time spoken of. They say in answer to the question, "Have you eaten?" "No." In answer to "Have you anything to eat?" they do not say "No," but "Nothing." The phrase "I have not eaten" involves again a different negative. Many phrases are of a double nature, like the French negative in "je *ne* sais *pas*." Altogether it is a highly elaborate tongue, with whose beauties few are acquainted. If rapidly spoken a sentence seems but one long word, so easily do the syllables flow together. The words themselves seem intricate changes on simple syllables; few double consonants unless at the beginning of a word, then generally of the extraordinary forms in "Mpembo," "Njeiŏ," "Msītŭ," "Nkōmbō," and as the preceding word ends in a vowel these readily combine. I do not think a dozen words in the language end in a consonant. The word for a cat is a suggestive one, "wai-ŏ," pronounced "why-ŏ."

"Analysis of one of the Trefriw Mineral Waters," by THOMAS CARNELLEY, B.Sc. Communicated by Professor H. E. ROSCOE, F.R.S., &c.

An analysis of this strongly ferruginous mineral water has not, so far as the author has been able to learn, been published before any scientific society; and though two general analyses of it have previously been made, the first by D. Waldie, Esq., in 1844, and the second by Dr. Hassall in 1871, and published in the form of pamphlets for public reading by Dr. Roberts and Dr. Hayward respectively, yet as it is peculiar for the extremely large quantity of iron and alumina that it contains, and as its composition has varied considerably since it was analysed by the last named chemist (whose results also varied from those of the first), it is thought that another and more complete analysis will not be out of place.

The village of Trefriw is situated on the left bank of the Conway about 2½ miles from Llanrwst and between the latter place and Conway. The springs, which now belong to a company and are often visited by invalids as they are said to be good for the cure of diseases of the digestive organs and of the skin, are close to the high road which runs between Conway and Llanrwst, and are rather over a mile from the village. The entrance to them is a short way up the side of the mountain called the Alt cae Coch, and consists of an underground passage cut in the rock. There are at present two springs (formerly there were three), one opposite and close to the entrance, the other at the end of a gallery 10 or 12 yards long to the right. The former water is used to supply the baths, and the latter exclusively for drinking; they differ considerably in the relative proportions of their mineral constituents, but it is only the last named which is the subject of this paper.

The water, which flows into a basin cut in the rock, is said to be uniform in quantity and issues at the rate of

about 40 gallons per hour; its temperature varies only within very narrow limits and is quite cold. As it occurs in the spring it is perfectly clear, bright, and colourless; but after a short exposure to the air it turns yellow and deposits flakes of ferric oxide; it has no smell, but possesses a strong and very disagreeable inky taste. On being shaken up in a closed bottle no disengagement of gas takes place; it has a strongly acid reaction, and contains neither free carbonic acid, carbonates, nor sulphides; and when first taken from the spring is perfectly free from ferric salts.

The following (I) is the analysis made of the water collected by the author on September 8th, 1874, together with that (II) made by Dr. Hassall in the early part of September, 1871,* or just three years previously.

Temperature of the External Air.........	15·5° C.
" " Air at the Spring...	12·5° C.
" " Water	11·0° C.

	I.	II.
Specific Gravity at 17° C........................	1·00716	1·00570
	Parts per 1,000,000.	
Loss on ignition....................................	7217·5	—
Precipitate formed on boiling 1 hour...........	32·8	—
Iron..	1507·0	2009·4
Aluminium..	233·3	112·4
Calcium	271·3	116·5
Magnesium..	134·1	45·4
Potassium	81·5	—
Sodium	25·1	15·3
Manganese	trace	trace
Lead	0·86	—
Ammonium (NH_4)	1·68	—
Albumenoid Ammonia	0·34	—
Silica (SiO_2)	157·0	149·0
Sulphuric Acid (SO_4)	4985·3	4512·0
Chlorine	11·8	10·9
Nitric Acid (NO_3)...................................	9·1	—
Phosphoric Acid (PO_4)	2·45	—
Total Solid Contents	7370·78	6970·9
The Residue dried at 310° C.	7370·00	—

* See "Guide to Trefriw and Vale of Conway Spa," by Dr. J. W. Hayward, M.D., M.R.C.S. Second edition.

The following table represents the above in combination:

	I.	II.
Ferrous Sulphate	4090·4	5454·3
Aluminium Sulphate	1358·9	700·7
Calcium Sulphate	922·3	376·0
Magnesium Sulphate	670·3	225·7
Potassium Sulphate	70·3	—
Sodium Sulphate	49·9	47·0
Lead Sulphate	1·25	—
Calcium Chloride	—	16·8
Sodium Chloride	19·4	—
Sodium Nitrate	4·8	—
Ammonium Nitrate	7·2	—
Aluminium Phosphate	8·2	—
Manganese	trace	trace
Silica	157·0	149·0
Albumenoid Ammonia	0·34	—
Bases for which there is not sufficient Acid	15·5	1·4 Loss
	7370·79	6970·9

With reference to this analysis the following observations are to be made:—

(1.) The determination of the total residue was first made at 180°C., as recommended by Fresenius,* and the result obtained corresponded to 8,100 parts per 1,000,000; it was found however that this was much too high, the reason being that ferrous sulphate, though it looses six molecules of water at 114°C., yet retains the seventh even at 280†. In order to drive off this remaining molecule, the residue from 100cc of water was heated in an air bath to 300°—310° and weighed; after repeated heating two successive weighings did not differ by more than a milligramme. In heating to so high a temperature, however, there is a danger of a little sulphuric acid volatilising by decomposition of the sulphate of iron, but by careful heating this may be avoided; a loss of ammonia will, nevertheless, have been incurred, but as this, together with the trace of organic matter, did not amount to more than 8 to 10 parts per 1,000,000, it was not of very much consequence.

* Fresenius. Quantitative Analysis. 4th Edition, p. 560.

† Watts. Dictionary of Chemistry. Vol. 5, p. 597.

(2.) It will be seen from the table showing the supposed combination of the salts, that the total bases formed were rather more than sufficient to combine with the acids, and the base which is given above as uncombined is alumina, as it is thought that the quantity of this body obtained was rather too high, for, in addition to the total bases being too large for the total acids, the sum of the oxides ($Fe_2O_3+Al_2O_3+P_2O_5$) calculated from the Fe, Al, and P_2O_5, each estimated directly, is rather greater than the result obtained by weighing the three oxides together, the numbers being 2,592 and 2,570 respectively—difference 22.

(3.) In the determination of the alumina it was separated from the iron by means of tartaric acid and sulphide of ammonium, and weighed as $Al_2O_3+P_2O_5$; the difference between this and the determined amount of P_2O_5 gave the quantity of alumina.

(4.) The phosphoric acid was estimated by precipitating with ammonium molybdate, and as the amount was only small, by weighing the precipitate obtained on a constant filter, the calculation was then made from the composition of the precipitate, which contains, according to various authorities, 3·142 per cent P_2O_5.

(5.) The iron was determined directly at the spring with potassium permanganate, and afterwards gravimetrically in the laboratory. The results obtained agreed very nearly.

(6.) Several determinations were made of the alkalies, but rather varying results, comparatively, could only be obtained for the sodium. The above is the mean of four, of which the highest was 32 and the lowest 22 parts per 1,000,000, the reason being that the quantity of sodium present was only very small, so that the traces of it also contained in the reagents had an appreciable effect, though they were as pure as could be obtained. The results got for the potassium, however, agreed very nearly.

(7.) The lead was determined by the method given in

Wanklyn & Chapman's Water Analysis, as were also the ammonium, albumenoid ammonia, and nitric acid.

By a comparison of the above two analyses it is evident that between September, 1871, and September, 1874, the composition of the water has varied considerably, and though the author has not had an opportunity of seeing the analysis made in 1844 by Waldie, yet from Dr. Hassall's report, given in the above-mentioned pamphlet, it would seem that the results there given also vary much from those obtained by Waldie. The quantity of iron appears to have greatly diminished, while, with the exception of SiO_2 and chlorine, that of the other constituents occuring in larger quantities has considerably increased. A determination of the iron made last February gave 1575·4 parts per 1,000,000, though in this case the determination was not made till after the water had been collected some days. From this it would seem that the iron is gradually diminishing in quantity. The result, however, obtained by Waldie is very nearly the same as that got by Hassall.

From the analysis it will be seen that the Trefriw water is peculiar, as already mentioned, on account of the large quantity of sulphate of iron which it holds in solution; there being, so far as the author has been able to learn, no spring in the United Kingdom, and perhaps not even on the Continent, which contains it in anything approaching to the same amount, while there are only a few springs known which contain it even in a notable quantity, the analyses of which have been described. The water is also remarkable for the large quantity of sulphate of alumina and silicic acid which are dissolved in it, while the phosphoric and nitric acids, though existing only in small amounts, are rather large compared with what is found in most other mineral waters; on the other hand the proportion of chlorine is only small.

The other Trefriw mineral spring was not analysed, but

from Dr. Hassall's analysis of the two waters, it appears to contain less iron and alumina, but a larger quantity of alkalies and alkaline earths than the one which is the subject of this memoir.

With regard to the geological position of Trefriw, and the source of the mineral impregnation of the springs, it may be observed that the mountains at the base of which the wells are situated consist chiefly of beds of limestone, ironstone, alum slate, and iron pyrites, together with varying proportions of silicates, very much fractured and dislocated, forming the northern extremity of the Bala or Caradoc beds. Up in the mountains and on these beds lie some small lakes from which the springs are supposed to derive their principal supply of water, which, after percolating through the above beds and dissolving large quantities of their constituents, finds its exit near the base of the mountain Alt cae Coch, where it issues from the slate bed (Black Band), and between it and the ironstone. From the above data the composition of the water is easily accounted for. There are several pyrites mines in the vicinity, one of which is situated just over the springs, but much further up the mountain side.

The author has been indebted for Dr. Hassall's analysis and some of his remarks relative to the geological position of the springs to the pamphlet of Dr. Hayward previously mentioned.

Ordinary Meeting, December 29th, 1874.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

"On a case of Reversed Chemical Action," by JAMES BOTTOMLEY, B.Sc.

Having observed the solubility of iodine in a solution of borax, an experiment was made to see what the result of this solution would be, expecting to obtain a combination of soda with excess of acid. 27·8475 grms. of borax were dissolved in about 250 grms. of water. As it was difficult to anticipate what the action of the iodine might be, this element was added at hazard, the quantity used being nearly seven grms. When assisted by heat almost the whole of this quantity dissolved in the solution, only a small quantity evaporating along with the aqueous vapour. The solution, which amounted to about 200 cc., had only a faint yellowish tint. Being set aside for some days, it deposited crystals which proved to be ordinary borax, for 0·5932 grms. of the crystals lost by heating 0·2773 grms. of water of crystallisation corresponding to 46·75 per cent, the theoretical quantity being 47·13. After removing the crystals the solution was still further evaporated in a retort. As the evaporation proceeded, instead of the faint yellow tinge disappearing as was anticipated, the colour of the solution began to darken, finally becoming opaque owing to the quantity of free iodine in solution; vapours of iodine were also given off along with the steam. Thus the iodine which had previously dissolved and chemically united with the soda when the solution was dilute, was displaced and eliminated in the free condition when the mixture was past a certain degree of dilution. The explanation of this reversal of

chemical action is as follows. When sodic borate is diluted with water its constituents are so far dissociated that the iodine acts towards the soda in the same way as it would towards caustic soda, sodium iodide and sodium iodate being the result. When however the solution is concentrated the boracic acid, notwithstanding its feebly acid power, is able to displace continuously and simultaneously small quantities of iodic and hydroiodic acid from combination with sodium, but these two acids cannot coexist in the free state; by mutual reaction they give iodine and water. To test the correctness of this explanation the following experiment was made. Boracic acid was added to a solution of sodium iodide; even after boiling some time the solution only acquired a feeble yellow tint, and no iodine vapours were given off; but on the addition of sodium iodate the solution soon became dull brown owing to the presence of free iodine, which also was given off along with the steam. This behaviour of iodine with sodic borate favours the view of the decomposition of the salt by dilution; it also shows the varying character of chemical affinity under different circumstances of temperature and dilution.

Ordinary Meeting, January 12th, 1875.

R. Angus Smith, Ph.D., F.R.S., Vice-President, in the Chair.

E. W. Binney, F.R.S., V.P., said that at the present time great attention was being paid to the sewerage of towns. Our Corporation had brought before the public a plan for dealing with the flood waters of the Medlock, and it is generally understood that Parliament will attempt to do something to prevent the further fouling of streams. Both Parliament and Corporate bodies have been long in moving in this

matter, and the doing of that which should have been done 30 years ago will now be a matter of the greatest difficulty. In 1840 the Irwell approached Salford in so pure a state that numerous fish were seen in its waters near the present Peel Park, but after Manchester and Salford sewers had entered it none were to be met with in it at Cornbrook. Fish were also at that time in the Medlock above Pin Mill Bridge.

In order to show how long a time authorities take to consider before they trouble themselves with action he brought a Report on the Streams of Manchester, published by Dr. Lyon Playfair in the Health of Towns Commission in 1844. After describing the geology, watersheddings, and streams of the district, the report proceeds as follows:— "When the boroughs of Manchester and Salford were not so thickly populated as at present and the surface of the soil was in its primeval condition, without any irregularities arising from artificial excavations, since caused by the making of bricks and for other economical purposes, the sites of the towns, from their undulating surfaces alone, would possess good top drainage into the streams and rivulets before mentioned. Some little obstruction to the drainage might arise from the rows of cottages which are generally built nearly at right angles to the inclinations of the surface of the land; still the irregularities produced by the hand of man have doubtless been in part rectified by public sewering into the watercourses above alluded to, and had such watercourses been allowed to pass unimpeded through the towns, although they might have caused some nuisances, still they would not be in anything like the filthy condition which they are now, owing to the weirs and dams marked X in map appended to this report that stop their courses and form so many cesspools, allowing some of the top waters certainly to flow along but collecting all the heavier particles, and thus continually generating the most offensive effluvia.

"The River Irwell, after having by its tributaries afforded drainage and sewerage to the towns of Bolton, Heywood, Bury, Rochdale, Radcliffe, and numerous other places, and having been pent up in countless reservoirs and dams for manufacturing purposes, approaches Salford by the Adelphi in a pretty tolerable condition as to purity, inasmuch as small fish live in its waters—a very rare circumstance in any other of the streams hereinafter mentioned. At the Adelphi is a high weir built quite across the river. After passing this impediment the stream is polluted by numerous works upon its banks and the contents of the sewers of the eastern and south-eastern parts of Salford, until it receives the waters of the Irk at Hunt's Bank in a much worse condition than its own—in fact, as filthy as water can well be; thence the river flows sluggishly along the western part of Manchester to Hulme, where it receives a portion of the waters of the Medlock and Shooter's Brook (a part of this being kept in the Bridgewater Canal) charged with the contents of the sewers of the eastern and southern parts of Manchester; it is then stopped at Throstle Nest by a dam across its stream. For many miles in its course towards Runcorn it emits offensive smells, and bubbles of light carburretted hydrogen gas rise to its surface.

"The Irk approaches Manchester from Blackley. It, like the Irwell, is anything but a pure stream to begin with. After being dammed up at Messrs. Appleton's paper mills, Mr. Hartley's dyeworks, Mrs. Crompton's paper mill, and Messrs. Appleton's upper logwood mills, it joins the Moston Brook at Collyhurst. This last-named brook is impeded in its course within a quarter of a mile of its junction by three weirs, namely, at Messrs. Appleton's St. George's logwood mills, Messrs. Dentith & Co.'s chemical works, and a weir not far from the old Rochdale Road at Collyhurst. Proceeding with the Irk:—This river after its junction with the Moston Brook is dammed up at Messrs. Appleton's lower

logwood mills and near the Bull's Head Inn in Newtown; and after receiving the refuse from the numerous dyeworks, size manufactories, chemical works, tanneries, skinyards, gasworks, sewers, &c., it is stopped at Messrs. Caistor and Thompson's corn mill at Scotland Bridge and the School Mills in Long Millgate before it reaches the Irwell at Hunt's Bank.

"The Medlock enters the borough at Beswick charged with the refuse of numerous works, and is dammed up by weirs across its course at Messrs. Guest's weir at Beswick, Neck Break weir, the Island Mill weir (a little below which near Fairfield Street it receives some fetid water from a small dam at Cruickshank's Mill), Messrs. Hoyle's weirs in Ardwick, and the weir near Messrs. Wood and Westhead's Mill in Garratt. At this place it receives the water of Shooter's Brook, a filthy little stream as black as ink, which enters Bradford from Newton, and after flowing under Butler Street to New Islington exposed is covered in till it reaches the Medlock at Garratt. This last named stream, after its junction with Shooter's Brook, passes under Oxford Road along Little Ireland, and is dammed up near Messrs. Birley's Mill in Kenyon Street. It then receives many sewers from the surrounding district and the river Tib in Gaythorn, which runs under the centre of Manchester but is entirely built over, and reaches the Duke of Bridgewater's Canal in as polluted a state as possible, thence some of its water after being employed as a power of winding goods escapes into its old course, which joins the Irwell near Hulme Hall. The waters of the Duke of Bridgewater's Canal are chiefly derived from the Medlock after it has received the contents of the Manchester sewers, and thus are in as bad a condition as the streams before described.

"Cornbrook enters Ardwick from Gorton, and partly open and partly built over traverses Ardwick, Chorlton-upon-Medlock, Greenheys, Hulme, and Cornbrook, at which

last mentioned place it reaches the Irwell in as polluted a state as any of the other streams.

"Besides the streams before described I may mention a little rivulet near Stock Street in Cheetham, which crosses York Street, and after forming several stagnant ponds enters the reservoir in Strangeways Park. There is also another rivulet, which crosses the New Bury Road at Stocks, and flows down to the pool near Strangeways Hall. A third goes from Cheetwood by the end of Broughton Lane; and a fourth by Broughton Grove. All these run into the Irwell. There is also a filthy and stagnant pool of water in front of the houses at Stony Knolls, which excites very little attention among the inhabitants. In Pendleton there is a small stream which, though it has often been presented at the court leets as a nuisance, and is correctly designated "The Black Ditch," remains in just as bad a condition as it ever did. In all the streams above described a number of dead dogs and cats are to be seen in the various states of decomposition, bubbles of gas, light carburetted hydrogen, rise up to the surface, and, although offensive smells are met with at all times, they are by far the most annoying when the barometer has experienced a sudden depression after having been high for a considerable time previously. Sulphurretted hydrogen is the gas which chiefly causes the odour, though doubtless phosphuretted hydrogen assists in some measure."

Such was the condition of the Manchester streams prior to 1844. Immediately after the publication of the Health of Towns Commissioners' Report the sewerage of towns and villages was increased to a great extent, and in nearly all cases the refuse matter was conveyed into the neighbouring streams instead of being utilised in manuring the land as it had been previously employed. No doubt sanitary engineers have been the chief offenders in polluting our waters during the last 30 years, but manufacturers have also done

their share by disposing of their refuse matter into the streams. The consequence is that not only are the waters more polluted, but their beds are being continually raised. Single towns on the banks of streams have little power to alter matters, there are so many vested interests to be dealt with. Numerous owners of property have by law what is called a right to foul waters by long user, and it will require strong parliamentary powers to effect any good. The owners of the numerous weirs will have to be compensated prior to such obstructions being removed and allowing the waters of the streams to flow and cut their courses as they once did. Before any great engineering works in the making of tunnels are attempted it is only reasonable that the streams should have a fair chance to cleanse themselves by their own natural flow of water. Manchester and Salford could try what the removal of the weirs at Douglas Mill, the Adelphi, and Throstle Nest would do. Surely such a simple experiment is worth trying before hundreds of thousands of pounds are spent in forming tunnels.

However, let Manchester and Salford spend what money they may, little good will be done unless the pollution of the waters above from their sources downwards to those towns is stopped. No doubt those towns set a bad example in the beginning, but as all the places on the streams are more or less guilty, they ought all to make a fair start together in the race of amending their evil ways of fouling streams and wasting manure.

If once the fœcal matter of a large town is diluted with water it is very costly to get it back again either by evaporation or sewage farming. Rival patentees advertise and recommend their respective plans as most efficacious, but at the present time he was not aware of the whole of the sewage waters of any large town in England having been profitably applied for farming purposes.

"On the Action of Rain to calm the Sea," by Professor OSBORNE REYNOLDS, M.A.

There appears to be a very general belief amongst sailors that rain tends to calm the sea, or as I have often heard it expressed, that rain soon knocks down the sea.

Without attaching very much weight to this general impression, my object in this paper is to point out an effect of rain on falling into water which I believe has not been hitherto noticed, and which would certainly tend to destroy any wave motion there might be in the water.

When a drop of rain falls on to water the splash or rebound is visible enough, as are also the waves which diverge from the point of contact; but the effect caused by the drop under the surface is not apparent, because the water being all of the same colour there is nothing to show the interchange of place which may be going on. There is however a very considerable effect produced. If instead of a drop of rain we let fall a drop of coloured water, or better still if we colour the topmost layer of the water, this effect becomes apparent. We then see that each drop sends down one or more masses of coloured water in the form of vortex rings. These rings descend with a gradually diminishing velocity and with increasing size to a distance of several inches, generally as much as 18, below the surface.

Each drop sends in general more than one ring, but the first ring is much more definite and descends much quicker than those which follow it. If the surface of the water be not coloured this first ring is hardly apparent, for it appears to contain very little of the water of the drop which causes it. The actual size of these rings depends on the size and speed of the drops. They steadily increase as they descend, and before they stop they have generally attained a diameter of from 1 to 2 inches, or even more. The annexed cut shows the effect which may be produced in a glass vessel.

It is not that the drop merely forces itself down under the surface, but in descending carries down with it a mass of water which when the ring is 1 inch in diameter would be an oblate spheroid having a larger axis of 2 inches and a lesser of about 1½ inches. For it is well known that the vortex ring is merely the core of the mass of fluid which accompanies it, the shape of which is much the same as that which would be formed by winding string through and through a curtain ring until it was full.

It is probable that the momentum of these rings corresponds very nearly with that of the drops before impact, so that when rain is falling on to water there is as much motion immediately beneath the surface as above it, only the drops, so to speak, are much larger and their motion is slower.

Besides the splash, therefore, and surface effect which the drops produce they cause the water at the surface rapidly to change places with that at some distance below.

Such a transposition of water from one place to another must tend to destroy wave motion. This may be seen as follows. Imagine a layer of water adjacent to the surface and a few inches thick to be flowing in any direction

over the lower water, which is to be supposed at rest. The effect of a drop would be to knock some of the moving water into that which is at rest, and a corresponding quantity of water would have to rise up into the moving layer, so that the upper layer would lose its motion by communicating it to the water below. Now when the surface of water is disturbed by waves, besides the vertical motion the particles move backwards and forwards in a horizontal direction, and this motion diminishes as we proceed downwards from the surface. Therefore in this case the effect of rain drops will be the same as in the case considered above, namely to convey the motion which belongs to the water at the surface down into the lower water where it has no effect so far as the waves are concerned, and hence the rain would diminish the motion at the surface, which is essential to the continuance of the waves, and thus destroy the waves.

"On the Stone Mining Tools from Alderley Edge," by Professor W. BOYD DAWKINS, F.R.S.

Discovery.

In May, 1874, Mr. H. Wilde and myself happened to take a walk to the new excavations which were in progress at the copper mines at Alderley Edge, which penetrate the rock on the east side of "the Street Road," leading to Alderley. The Lower Keuper sandstone in that place is impregnated with carbonate of copper, in search of which tunnels had been driven into the base of the hill in the main parallel to the strata, having there a depth to the west of about 29°. In following the ore from the deep upwards the miners had worked their way to the surface, on the hillside immediately above the heaps of refuse near the reducing tanks, and laid bare a considerable portion of the rock. While walking over this surface, which was fantastically hollowed, a worked stone happened to catch

my eye; and when we examined the stones lying about in the hollows we saw at once that a large number had been used in mining operations; and of these, owing to the kindness of the manager and the captain of the mine, we were able to secure thirty-five, which are now lodged in the Museum at the Owens College.

Description of Tools.

These mining tools are divisible into three classes: 1, the hammers with a simple groove round the middle for the retention of the withy which formed the handle; 2, the hammers which besides this groove have one of their ends also grooved for the reception of another withy, and thus were prevented from slipping when a blow was struck; and lastly, there were two implements which probably had been used as wedges, being possessed of an edge blunted by wear, and exhibiting marks of having been struck on the other. One of these has a surface which looks as if it had been glaciated, and the second, in shape very much like a celt, is remarkable for the clear evidence which its surface offers, that the groove around it for the reception of the withy was cut *after* the stone had been ground to its present shape, and probably long after, in consequence of the decomposition of the surface of grinding as compared with that of the groove.

All these implements were derived from the ice-borne stones of the boulder clay, of which they were merely picked specimens which happened to be useful for the special purpose of mining.

Tools found in old Surface Workings.

Subsequently, in the autumn of 1874, many more specimens were obtained by Col. Lane Fox and myself through the kindness of Lord Stanley of Alderley and the manager of the mine, and we were able to make a careful examina

tion of the conditions under which they were found. To pass over those which have been buried, the number which I have examined is considerably over one hundred, belonging to the three types mentioned above.

The rock where the tools were met with was hollowed out irregularly and evidently artificially, and to a depth in some cases of from 8 to 11 feet from the surface. And from an examination of the ground it was perfectly obvious that the ancient users of these tools had worked the metalliferous portions from above, without attempting to make galleries. The tools lay buried in the *débris* which had been thrown into the old surface workings after they had been discontinued, and which presented all the characters of "a wheelbarrow formation," and were found in the greatest abundance near the bottom.

Comparison of Stone Hammers with those of other Districts.

Stone hammers of the kind mentioned above, and especially of the simple grooved class, are very widely distributed. They have been found equally in the ancient copper mines of Anglesea, of Spain and Portugal, and of Lake Superior. With these also the Egyptians worked the turquoise mines of Wady Magarah, in the Sinaitic peninsula. They undoubtedly represent one of the ruder and probably earlier stages in the art of mining. With the solitary exception offered by the turquoise mines at Magarah, they have only been discovered in old copper workings, and they may therefore be inferred to have been used in ancient times mainly for the extraction of that metal.

No Direct Evidence as to Date.

I will not venture to attempt to assign a date to the mining operations carried on at Alderley, when these imple-

ments were in use. In all the ancient mines, worked by the Romans, so far as I know, iron tools have alone been met with. Nor am I aware of any mines, of post-Roman date in Europe, which have been carried on with tools composed of any other material. It would, therefore, seem probable that they are of pre-Roman age, and that they are of the class termed pre-historic by the archæologists.

What Ores were sought in the Surface Workings.

Nor is it absolutely certain what metal was sought in these surface workings, because ores of copper, cobalt, lead, iron, and manganese are associated together in that spot. If they were in search of copper, the ore must either then have been richer than that which they left behind, or they must have been acquainted with some mode of reducing the small per centage of copper (which averages considerably less than 5 per cent) from the matrix, of which we are ignorant. This is at present effected by a bath of hydrochloric acid. Possibly, like some of the joint-stock companies of the present day, they may have been seeking for copper without success; but in that case the large number of stone hammers is not explained. Had tools such as these been used for the extraction either of lead or of iron they would most probably have been discovered in the workings which have been carried on throughout Great Britain, certainly since the Roman occupation to the present day. And it is hard to believe that the miners of Alderley worked these metals in a ruder fashion than any others in this country, so far as the present evidence stands. Nor is it at all likely that the insignificant and obscure ores of lead and iron at Alderley would attract the notice of miners in ancient times, when both were obvious, and very rich in the adjacent districts of Lancashire and Derbyshire.

The only conclusion which I will venture to draw, is that these implements imply a ruder phase of the art of mining

than has hitherto been known in the neighbourhood of Manchester—a phase which may point back to the bronze age, when the necessary copper was eagerly sought throughout the whole of Europe.

"Archaic Iron Mining Tools from Lead Mines near Castleton," by ROOKE PENNINGTON, LL.B.

The iron and wooden mining tools now submitted for the Society's inspection are from the Mock mine, a lead mine in the High Rake, a vein of ore in the Tideslow liberty, about four miles to the south of Castleton. They were found in old workings at between 80 and 90 yards below the surface. According to the information I have been able to collect in the neighbourhood, this mine has not been worked for more than 200 years, but I cannot say that this is entirely to be relied upon. Probably however a minimum of 200 years may be taken as the age of the tools, independently of such evidence, for the following reasons. In the first place, a similar collection of tools from another part of the same "rake" is very well described by Mr. Benjamin Bagshawe at p. 43, No. 13, vol. 3, of the "Reliquary." These tools were found associated with silver coins of the time of Charles I, and two tradesmen's tokens dated 1667.

Again, the tools I am describing were accompanied by a particular form of small tobacco pipe, which according to Mr. Llewellynn Jewitt, F.S.A. (Reliquary, October, 1862, p. 75), is probably Elizabethan, and certainly not later than Charles I. These pipes are usually known in Derbyshire as "fairy pipes," or sometimes (with great correctness) as "old man's pipes," the "old man" being the term used to describe the miners of past ages and by transposition applied to the mines themselves, so that old workings are usually called the "Old Man."

An amusing mistake once arose from this confusion of terms. A miner from the Peak giving evidence in London

on a mining case, continually referred to the "Old Man," until the opposing counsel objected that this second-hand evidence ought not to be admitted, but that the individual referred to should himself be called as a witness. It is also an illustration of the untrustworthy character of popular names that these pipes are also known as "carl's pipes," just as a prehistoric hill fort near Hathersage is called the "Carl's Wark."

It will be observed that one of the tools has been a wooden spade, tipped with iron. This serves to show the value iron possessed, or rather its comparative scarcity. All the other tools are altogether of iron, except the handles, but I do not on this account regard the spade as of earlier date, inasmuch as nothing but metal would serve the purpose in the other tools, whilst a mere shovel might do very well although made of wood.

All the tools are of archaic type, though some are recognised by old miners as similar to those in use in their youth.

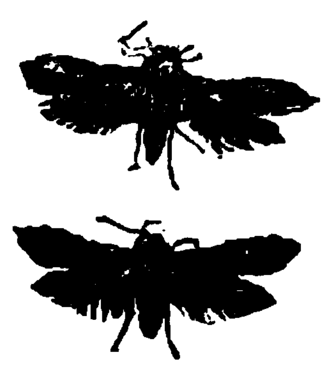
MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 7th, 1874.

Professor W. Boyd Dawkins, F.R.S., in the Chair.

Mr. James Cosmo Melville, M.A., F.L.S., was elected Secretary of the Section, in place of Mr. Sidebotham, resigned.

JOSEPH SIDEBOTHAM, F.R.A.S., read a paper on *Œcophara Woodiella* (Curtis). This moth was discovered on Kersall Moor in the year 1829, and figured and named by Curtis in 1830, since which time it has not been again met with,



either in this country or abroad. The only specimens known to exist are the one from which the original drawing was made, and a pair in the museum of Owens College. The author exhibited the last-mentioned specimens, also drawings from the various published figures of the species, as well as photographs of the natural size from the pair belonging to the College.

Ordinary Meeting, January 26th, 1875.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

John Dixon Mann, M.D., M.R.C.S., was elected an Ordinary Member of the Society.

"A Descent into Elden Hole, Derbyshire," by Rooke Pennington, LL.B.

Near the road from Buxton to Castleton, and about four miles from the latter place, stands Elden Hill. It is a bleak, bare, mountain, one of the highest of the grassy eminences of the Derbyshire limestone district, and, though uninviting itself, commanding a most extensive and fine view. Its summit was long ago in the prehistoric past chosen as a resting place for the bones of some savage chief, and in its side is Elden Hole.

Elden Hole is a perpendicular chasm in the rock, and like many such apertures is reputed to be bottomless; and indeed the sound of a stone falling into the gulf is almost enough to justify the reputation, for, bounding from side to side and smashing as it bounds, the noise of each leap gradually diminishes, but the stone is never heard to stop. Though still famous, it is not so famous as it once was. It was once reckoned one of the seven wonders of the Peak. In the time of Queen Elizabeth the Earl of Leicester is said to have let a man down into it who was drawn out speechless and who shortly afterwards died. A cat subsequently lowered a considerable distance was also brought out dead. A hundred years later, Cotton, the poet of the Peak, tried unsuccessfully to fathom it; he has recorded his experiment in verse. At length, just a hundred years ago, Mr. Lloyd, F.R.S., went to the bottom himself and returned safely. His account of what he saw is printed in the Philosophical Transactions. But Mr. Lloyd was a man before his time,

and the country-side still went on believing in Leicester's speechless man and dead cat rather than in the scientific explorer, and Elden Hole remains an unfathomable abyss to this day in the belief of the peasant and the tourist, and its glory has departed not through diminished depth, but because it is near no railway and even adjoins no carriage road, and the nineteenth century traveller rarely visits beauties or wonders the way to which can only be travelled on foot.

On the 11th of September, 1873, we explored the chasm for ourselves.

A number of stout beams and planks had been brought up the day before, and of these a rude platform was constructed. We found it was impossible to make this platform and place our windlass so as to obtain a descent plumb to the bottom, or rather to the first landing place in the chasm, inasmuch as the northern end is the only part where such a drop can be obtained, and there the gulph was much too wide to be bridged over. Having made all our arrangements, we commenced our descent. My friend Mr. J. Tym of Castleton was the first to go down. He was let down for about 15 or 20 yards before coming in contact with the projecting side of the gulf. For about another 10 or 12 yards he slipped over the rock, which was however perfectly smooth, so that there was no risk of cutting the rope. He sustained no further injury than that which befell those of us who followed him, viz., a complete rolling in mud derived from the damp and slippery rocks. As the pioneer, however, he ran considerable danger from stones which had lodged on ledges of rock and which there was risk of disturbing. When a little more than half way down he came clear of the rock again, and there was a sheer descent to the bottom, the rope continuing to run over the smooth projecting side. Three of us followed him, one at a time, each of us being tied to the rope so as to have the hands free to guide the body.

The effect of being lowered into the dark abyss, with the blue sky above and the green ferns and creepers around, was very fine, but the knocking one got against the rock a few yards down soon distracted the attention from scenic effect. At a distance of 180 feet from the top a landing place was reached, although not a very secure one, as it was inclined at an angle of about 45 degrees. Thence a cavern ran downwards towards the south or south-east; the floor was entirely covered with loose fragments of limestone, probably extending to a considerable thickness.

This is no doubt to some extent natural, but principally artificial, being the result of the favourite amusement of visitors, the throwing down of loose stones from the projecting wall which surrounds the top. The farmer told me that during his time two or three walls had disappeared and been replaced. No doubt it had been the same in time of his predecessors. There was quite sufficient light at this point to enable one to sketch or read. Having refreshed ourselves we left daylight behind, and scrambled, or rather slipped, into the cavern for some few yards, during which we descended a considerable distance; it was of a tunnel-like shape; then it suddenly expanded into a magnificent hall about 100 feet across and about 70 feet high. The floor of this hall sloped like the tunnel, and like it was covered with *debris*. At the lower side we were about 60 feet below our landing place, and therefore about 240 feet beneath the surface. The entire roof and walls of this cavern were covered with splendid stalagmitic deposits. From the roof were hung fine stalactites, whilst the sides were covered with almost every conceivable form of deposited carbonate of lime. In some places it was smooth and white as marble, in other places like frosted silver, whilst the rougher portions of the rock were clothed with all sorts of fantastic shapes glistening with moisture. When we had lighted some Bengal fire, I need hardly say the effect was

exquisite. From this cavern we could find no opening of any length or depth save the one by which we had entered it, although we very carefully explored it. There is an absurd local tradition of an old woman's goose which flew down, and was given up as lost, but which subsequently re-appeared at the mouth of the Peak Cavern, at Castleton. The sagacious and enterprising bird must have been much cleverer than we were.

There can be no doubt that this chasm has been formed by the chemical action of carbonic acid in water, and that it has attacked this particular spot either from the unusual softness of the rock originally situated here, or because there was here a joint or shrinkage in the strata. There is nothing, however, in the position of Elden Hole to lead one to suppose that any stream has ever flowed through it; no signs of such a state of things appear anywhere around.

In this it differs from the numerous water swallows of the neighbourhood, and from the pot-holes of North-West Yorkshire. It is not related to any valley or ravine, or to any running water, and there is, as observed, an absence of any well defined exit for water at the bottom. No mechanical action of a flowing stream can therefore have assisted the process of enlargement.

That so deep a chasm should be entirely isolated is certainly remarkable, because it cannot be said to represent a "weak point towards which the rainfall has converged." It must, therefore, be due to the gradual silent solvent properties of rain-water falling on the surface, and escaping through jointings and insignificant channels in the hard rocks below. Whether the excavation took place from above or below is uncertain. Applying the rule "a ravine is a cavern open to the sky," such an abyss is simply a perpendicular cave whose roof has at length fallen in. But there are many shallow funnel-shaped depressions in the limestone to which this rule will not apply, and which have

been begun from the top, and there are also holes of very irregular shape, apparently of downward origin. So far as the perpendicular shaft of Elden Hole is concerned, it is not at all unlikely that the excavation descended from the surface; on the other hand, there are many underground chasms which have probably been excavated upwards, and which when uncovered will present very much the appearance of Elden Hole. For example, the upper part of the great chasm in the Speedwell Mine bears a strong resemblance to it, though the process is not there yet complete, and could sections of the district south of Castleton be made, these hollow mountains would probably reveal many such a cavity.

SECTION OF ELDEN HOLE, DERBYSHIRE, SEPTEMBER, 1873.

SCALE 100 FEET IN AN INCH.

"Certain Lines observed in Snow Crystals," by ARTHUR W. WATERS, F.G.S.

The crystalline form of water belongs to the hexagonal system, and the best condition, but not the only one, for observing this is as snow crystals, or perhaps more correctly as ice crystals, for with a very low temperature there are frequently beautiful ice crystals floating in the air when there is no snow-fall.

Of such some snow is formed but not all. Dr. Nettis, of Middleburg, published some drawings in 1740. Scoresby*

* Scoresby. Account of the Arctic Regions.

in 1822 figured about one hundred forms from the Arctic Regions, and Glaisher in 1844 gave some very elaborate drawings in the Journal of the Microscopical Society. The figures from both these two last sources have been very largely copied for various text books and scientific works, and are probably familiar to most present.

These figures are made up of angular lines, which seems quite natural for crystalline forms. Some such ice crystals* which I observed and figured from Davos in Switzerland, show a variety of curved lines, the cause of which I found for some time enigmatical, but the explanation is so extremely simple that I feel surprise that it did not at once present itself to me, and the explanation I believe shows that such observations may have a scientific utility.

It is not often in England that there are favourable opportunities for observation, as the temperature seldom is much below freezing. Frequently in the Swiss mountainous districts when a very low temperature obtains there are a great number of the ice crystals just mentioned floating in the air. These are usually the most beautiful and regular, and are the most readily examined. The illumination I used was Wenham's Parabolic Reflector.

FIG. G.

The lines which I wish to bring before your notice are the curved internal ones, especially such as the meander line in fig. G. This crystal I carefully watched during the process of melting. First the external arms melted down as far as this meander line; next the crystal melted down to the straight sided hexagon; this then melted further, leaving a crystal with the shape of the next interior lines. When we see it taken to pieces we may judge that it was built up in somewhat

* Klimatologische Notizen ü. d. Winter im Hochgebirge von Arthur Wm. Waters, Basel, 1871.

the same manner, that is to say by a growth from one form to another, so that on the simple tabular hexagon, growth takes place along the six sides, or from the corners arms may be thrown out, and such changes may, as my figures show, be repeated several times. But to explain the curved line found in the interior we must examine a little further. When a crystal begins to melt it loses the angular corners and gradually melts down, so that we can suppose a crystal, G, thawing down to take the shape of the meander line. This brings us to the explanation of these curved lines, which indicate in the first place a thawing, but as the arms extend beyond these lines it is clear that after this partial thawing freeziug has begun again.

The production of these lines thus shows that the crystals, after having been formed in a cold atmosphere, have passed into a warmer one, where part of the crystal has been removed by thawing; after this it has again passed into colder air, and has formed anew upon the partially melted crystal.

It seems to me that these crystals bring down with them registers of the comparative temperature of regions inaccessible to the meteorologist, and that a systematic examination in favourable localities would add much to the knowledge of this science, as we may thus learn where counter currents exist. To take the crystal G, which fell on the 22nd of December, 1870, the meteorological observations for Switzerland show that the prevailing wind east of Davos was north or north-east, with thermometer falling, while to the west the wind was south and west. The conclusion which I have arrived at from the lines under consideration is that above Davos a layer of this southerly wind intrudes into the colder northerly winds, and the observations of other days do not militate against this theory; but, as I have been able to compare but few days, I should be glad to see a series of observations undertaken to obtain further information.

In Switzerland, the wind observations are taken by the anemometer placed near the meteorological station. Now many of these are in the valleys, and as the valley winds are so frequently merely local, these observations have little value for our purpose, and the same may be said for general meteorological purposes. On January 9th, when some of the crystals were observed, the wind registered in the valley at the meteorological station was north, while the clouds, which I always noticed for my observations, showed that a south wind was blowing above.

I have called attention to the fact that I did not find the classification of Scoresby* always corresponded with what I observed; finding sometimes low temperature forms on the coldest days. This may probably be explained by the fact that in the arctic regions there are fewer counter currents, and so the crystals were formed under more uniform circumstances; yet at the same time the only explanation of the variety exhibited in each crystal must arise from growth taking place under slightly altered circumstances, or else why should it suddenly change from one type to another?

The amount of wind seems to exert quite as much influence upon the form of the crystal as does the temperature. When there is little motion of the air the crystal can go on building itself up regularly in all six directions; but with a considerable amount of wind the conditions for this building up are more favourable on one side than another, so that we may get showers of acicular snow when a strong wind prevails. On the same principle the one side of a rope may be covered with hoar frost turning in the direction of the wind, but the same thing can sometimes be still better seen on a fir, all the leaves of which are covered with acicular hoar frost; these needles in the same way all having a direction against the wind.

The temperature when the crystal figured fell was between $-10{\cdot}5$ C. and $-11{\cdot}4$ C.

* Voyage to Greenland, 1823.

Ordinary Meeting, February 9th, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. R. F. Gwyther, B.A., Lecturer on Mathematics at the Owens College, and Mr. M. M. Pattison Muir, Demonstrator in the Chemical Laboratory of the Owens College, were elected Ordinary Members of the Society.

"A Method of finding the Axes of an Ellipse when two conjugate diameters are given," by J. B. MILLAR, B.E. Communicated by Professor O. REYNOLDS.

In Mechanical Drawing it is frequently required to draw an ellipse of which two conjugate diameters are given, or, what comes to the same thing, to inscribe an ellipse in a given parallelogram.

The following, which so far as I know is new, is a simple method of solving this problem by obtaining the axes of the ellipse, from which it may be described by means of a trammel or any other of the well known methods.

Let AB and CD be two conjugate diameters of an ellipse; it is required to determine its axes.

Construction.—From A, the extremity of the longer diameter (or from the extremity of either if they be equal), draw AE at right angles to OD and make AF equal to OD: on the line OF as diameter describe a circle; join A with the centre G and produce AG to meet the circumference in K. OH and OK will be the directions of the major and minor axes respectively, and

OL=AK=semi-major axis.

OM=AH=semi-minor axis.

Proof.—The ellipse of which OL and OM are the semi-axes will evidently be traced by the point A if the line AK be made to move so that H and K move along the axes. It only remains therefore to show that this ellipse will pass through the point D and have AB and CD for conjugate diameters.

It is well known that when a circle rolls inside another of twice its diameter, every point on the circumference of the rolling circle moves along a diameter of the other. If then the circle OKH be rolled inside the circle FNP the points H and K would move along OH and OK so that the point of which A is the initial position, moving with the circle, would describe the ellipse of which OL and OM are the semi-axes. Now as the circle OHK rolls inside FNP the points E and F move along the diameters EC and FP;

so that when F has moved to O, FA will coincide with OD and the point A with D, since AF is equal to OD.

Therefore the ellipse will pass through D.

Again, as the circle OHK begins to roll from its present position F will be the instantaneous centre, and therefore the tangent to the curve at the point A will be perpendicular to AF, that is parallel to OD, since AE is perpendicular to OD.

Therefore OA, OD are conjugate diameters of the ellipse of which OL and OM are the semi-axes.

The PRESIDENT called attention to a paper in Poggendorff's Annalen for July, 1872, by H. Abich, on a fall of hail of a remarkable character which took place near Tiflis on the 9th of June, 1869.

E. W. BINNEY, F.R.S., V.P., presented to the Society a bust of the late James Wolfenden, of Hollinwood, one of most noted Mathematicians of the Lancashire school, who was born on the 22nd June, 1754, and died on the 29th March, 1841. In vol. 50, p. 387, of the *Mechanics' Magazine*, 1849, is a memoir of the deceased, written by the late Mr. T. T. Wilkinson, F.R.A.S., a corresponding member of the Society, who states that Mr. Wolfenden was sent to a day school at the age of six years, but the bobbin wheel and loom being considered much more profitable employment than learning to read, he was taken away after one week's attendance, and the sum of three halfpence defrayed the expenses of his scholastic education. Those deficiencies were in some degree supplied by the assiduity of his grandfather, who took advantage of the intervals of leisure, after the day's weaving, to instruct him in reading, writing, and arithmetic. From this stage Mr. Wolfenden may be

said to have been self taught, if we except some occasional assistance he received from Mr. Jeremiah Ainsworth, a well-known mathematician, then resident near Hollinwood. Though his days were occupied at the loom, he spent most of his leisure hours in reading all the works on science he could procure in that then thinly populated neighbourhood, so that at the time he arrived at manhood he was well acquainted with most of the writers on physical and mathematical subjects, and had made the works of Euclid, Newton, Simpson, and Emmerson, his particular study. In 1807 Mr. Wolfenden calculated the first Tide Table for the port of Liverpool, which was published by Mr. Lang in the "Original Liverpool Almanack." In this work he proposed and solved the following problem:—"Suppose the sun and moon in the equinoctial, and the ratio of their forces to raise the tides to be given, it is required to find, *geometrically*, their elongation when the interval or intercepted arc between the place of high water and the moon is the greatest possible."

The solution is founded on the lemma to proposition 58, "Simpson's Select Exercises," and shows how much can be effected by geometry, when applied by a skilful hand. In a foot-note he informs his readers that Bernouilli and other writers on the theory of tides, make use of the fluxions in the investigation of this problem.

The bust was given to Mr. Binney by Mr. Wm. Hadfield Bowers, of West Gorton, who received it from his father-in-law, Mr. Whitaker, an old friend of Mr. Wolfenden.

On the motion of Mr. Bailey, seconded by Professor Reynolds, the thanks of the Society were voted to Mr. Binney for his donation of a bust of the late Mr. James Wolfenden.

PHYSICAL AND MATHEMATICAL SECTION.

February 2nd, 1875.

ALFRED BROTHERS, F.R.A.S., President of the Section, in the Chair.

"Results of Meteorological Observations taken at Langdale, Dimbula, Ceylon, in the year 1873," by EDWARD HEELIS, Esq. Communicated by JOSEPH BAXENDELL, F.R.A.S.

TEMPERATURE.

	Mean Temp. of the Month.	Mean Daily Range.	10.30 a.m.			6 p.m.		
			Air.	Evap.	Diff.	Air.	Evap.	Diff.
January	64·35	18·1	67·6	55·9	11·7	66·5	57·3	9·2
February......	65·90	18·8	70·2	59·1	11·1	66·5	59·8	6·7
March	67·60	23·0	74·5	60·6	13·9	66·2	59·6	6·6
April	68·40	20·0	74·4	65·1	9·3	66·6	63·8	2·8
May............	67·65	17·5	73·3	66·4	6·9	66·2	64·4	1·8
June............	64·70	6·6	65·3	63·9	1·4	63·2	62·1	1·1
July............	63·90	8·0	64·9	62·6	2·3	62·7	61·4	1·3
August	66·05	11·1	67·5	64·1	3·4	65·9	64·3	1·6
September ...	65·85	14·3	67·2	62·3	4·9	64·1	61·6	2·5
October........	65·00	11·0	65·9	64·4	1·5	63·3	61·8	1·5
November	66·90	15·2	70·4	63·6	6·8	64·8	62·5	2·3
December.....	66·50	15·2	70·1	61·9	8·2	65·0	62·5	2·5
Means......	66·06	14·9	69·3	62·5	6·8	65·1	61·8	3·3

RADIATION.

	Mean Max. in Sun.	Max. in Sun less Max. in Shade.	Max. in Sun Ther. in vacuo.	Min. on Grass.
January	99·5	26·1	129·4	44·1
February.........	106·7	31·4	137·1	50·2
March	118·8	39·7	146·8	49·5
April	116·3	37·9	151·0	52·3
May...............	117·9	41·5	143·7	54·3
June...............	93·1	25·1	116·6	59·6
July................	98·4	30·5	126·1	57·4
August	109·6	38·0	134·2	58·2
September.......	113·7	40·7	140·1	54·7
October...........	106·0	35·5		56·5
November	115·5	41·0		53·9
December........	116·2	42·1		52·4

RAIN, CLOUDS, AND OZONE.

	RAIN.			CLOUDS.		OZONE.		
	Amount	No. of Days.	Greatest Fall in 24 hours.	10-30 a.m.	6-0 p.m.	Mean	Least Daily Amount	Greatest Daily Amount
January	0·40	3	0·17	3·4	4·1	7·1	3	9
February	5·60	10	1·47	4·7	6·3	7·4	5	9
March	3·47	8	1·17	1·9	4·6	6·6	3	9
April	8·61	13	1·55	3·7	6·7	6·2	5	9
May	11·89	22	2·45	4·0	8·4	6·4	4	9
June	26·98	30	2·65	9·0	9·9	6·5	5	8
July	19·59	26	2·55	8·7	8·8	5·8	3	9
August	12·89	26	1·31	5·5	8·6	5·3	3	9
September	11·40	15	3·13	5·1	7·7	4·9	2	8
October..................	10·24	25	1·26	7·9	8·9	5·6	3	9
November	5·90	16	1·62	6·2	7·2	5·6	3	9
December...............	6·43	16	2·75	5·3	7·1	5·7	4	8
	123·40	210		5·4	7·4	6·2		

The warmest month was April, and the coldest July, but the difference of mean temperature was only 4·5 degrees. The greatest mean daily range was 23·0 degrees in March, and the least 6·6 degrees in June. The mean humidity of the air, at 10.30 a.m., was greatest in June, and least in March; at 6 p.m. it was also greatest in June, but least in

January. In October, however, it was nearly as great as in June, both at 10.30 a.m. and 6 p.m.

The difference between the mean maximum temperature in the sun and the mean maximum in the shade was greatest in *D*ecember, and least in June. The values for May, November, and September differed, however, but little from that in December, while the value for January was only one degree above that for June. The low value for January is remarkable as the dryness of the air was above the average, the rainfall at a minimum, and the mean amount of clouds much below the average.

The mean minimum temperature on the grass was lowest in January, the month of least amount of cloud at 6 p.m., and highest in June, the month of the greatest amount of cloud at the same hour. The lowest minimum on the grass was 33·0 degrees in January, and the highest 57·0 degrees in June.

The greatest monthly rainfall was 26·98 inches in June, and the least 0·40 inches in January. The six consecutive months of greatest fall were May to October, when the total amount was more than three times that of the remaining six months.

The greatest mean amount of ozone was 7·4 in February, and the least 4·9 in September, the difference being thus only 2·5. Ozone was present on every day of observation, and the lowest daily amount recorded was 2·0 in the month of September. In February, April, and June the daily amount never fell below 5·0. On the other hand the daily amount never exceeded 9·0 in any month.

In every month the mean amount of cloud at 10.30 a.m.

was less than at 6 p.m., which is contrary to what usually happens at most places in England.

On the 9th of December at 3 p.m. a miniature cyclone occurred on a portion of the estate at Langdale. Its diameter was about 200 yards; rate of progression say 4 miles per hour; and the velocity of the wind say 70 miles per hour. Bare branches 2 inches thick at butt and 6 feet long were carried some 50 feet high to a distance of at least 300 yards.

Ordinary Meeting, February 23rd, 1875.

R. ANGUS SMITH, Ph.D., F.R.S., Vice-President, in the Chair.

Mr. JOSEPH SIDEBOTHAM, F.R.A.S., sent for exhibition a specimen of the Colorado Potatoe Beetle (*Doryphora decomlineata*), which had appeared in great numbers in Canada last year, and had caused great destruction in the potatoe crops.

E. W. BINNEY, F.R.S., V.P., exhibited to the society specimens of a strong arenaceous shale, approaching to a flagstone, containing numbers of macrospores of *Lepidodendron.* Several times previously he had brought before the Society similar specimens in coals, and showed that they formed a considerable portion of the Fifeshire splint, but he had never previously found them in arenaceous shale or sandstone, although he had often looked for them. Many years since Professor John Morris, F.G.S., in Vol. V., part 3 (1840), of the Transactions of the Geological Society of London, in a catalogue of the fossils mentioned in Professor Prestwich's Memoir of the Coalbrookedale Coal Field states that "capsules neither bitumenized nor mineralized, but in a state of brown vegetable matter, are very abundant in some of the coarser sandstones of the coal measures." He collected the specimens from a new pit at Woodbank, near Methel Hill, Fifeshire, where a winning was being made down to the Cameron Bridge Coal, and his attention was first directed to them by his friend Mr. J. W. Kirkby, of the Pirnie Colliery. Their great abundance in seams of coal was considered very remarkable, but when they are also found largely in arenaceous shales and sandstones associated with such coals it clearly shews that the plant of

which they are the organs of reproduction must have contributed very largely to the production of such beds of coal. They doubtless were floated in water with other vegetable remains to the places where they are now found, and owing to their coriaceous covering have been preserved whilst their accompanying plants have only left traces of their remains in the black charcoal dispersed throughout the shale. The macrospores are compressed, but their upper surfaces are minutely tuberculated, and their under ones marked by a tri-radiate ridge. They yet contain sufficient combustible matter to afford a brilliant flame in a burning candle.

Ordinary Meeting, March 9th, 1875.

EDWARD SCHUNK, Ph.D., F.R.S., President, in the Chair.

"On Mr. Millar's Method of finding the Axes of an Ellipse when two conjugate diameters are given," by ROBERT RAWSON, Esq., Honorary Member of the Society.

At the ordinary meeting of the Society, February 9th, 1875, Professor O. Reynolds communicated a paper on "A Method of finding the Axes of an Ellipse when two conjugate diameters are given," by J. B. Millar, B.E.

Of this solution of an ancient and useful problem it is necessary to observe that it is accurate, simple, but not new.

It may be found in Waud's Algebraical Geometry, art. 290, page 139. The construction here referred to is the same as that employed by Mr. Millar, with the exception of the generating circle OKEF, which, however, is not necessary in the construction of the principal axes. Waud's solution depends upon the well known property, viz., the equal areas of all parallelograms whose diagonals are conjugate diameters in position and magnitude.

The same construction may readily be inferred from Professor Lowery's solution of a problem by Sir James Ivory, in Leybourn's Mathematical Repository, vol. I, new series, page 175.

Three solutions of this problem are given by Mr. Besant in his Geometrical Conics—art. 216, art. 217, and art. 249, Appendix. The first two of these solutions are not so simple as is the third, which would in all probability have been the same as Mr. Millar's had he taken PE in his figure in the opposite direction. Mr. Besant derives his construction from the investigation of the locus of a fixed point in a given straight line whose extremities move on the legs of a right-angled triangle.

The various properties of the ellipse on which the solution of this problem may depend are as follows, and they may be found demonstrated in most modern works on conic sections :—

The lines AA′ BB′ are the principal diameters, CC′ PP′ are conjugate diameters, PF is perpendicular to OC, meeting the axes in G and g, PT is a tangent at P, and therefore parallel to CC′. At' A′T′ are tangents at A and A′.

$PF \,.\, PG = OB^2$ ………(1)	$PF \,.\, Pg = OA^2$ ………(2)
$PG \,.\, OA = OC \,.\, OB$ …(3)	$Pg \,.\, OB = OC \,.\, OA$ …(4)
$PG \,.\, Pg = OC^2$ ………(5)	$PF \,.\, OC = OA \,.\, OB$ …(6)
$PT \,.\, Pt = OC^2$ ………(7)	$OA^2 + OB^2 = OC^2 + OP^2$ ..(8)
$At' \,.\, A'T' = OB^2$ ………(9)	$SP \,.\, S'P = OC^2$ …….(10)
$Pt' \,.\, PT' = OC^2$ …….(11)	

The properties 7, 8, 9, and 10 are true for any conjugate AA′ and BB′.

Emerson, in his Conic Sections, published in 1767, solved this problem by means of property 7. Sir J. Leslie, in his Geometry of Curved Lines, pages 255, 256; and Salmon, in his Conic Sections, art. 179, page 168, fifth edition, have repeated Emerson's solution.

The radius of the generating circle used by Mr. Millar is determined in terms of the principal axes by Wallace in his Conics somewhat in the same manner as that used by Mr. Millar.

This problem is given as an exercise by C. Taylor, in his Conics, published at Cambridge in 1863, ex. 9, page 90.

Several eminent writers do not refer to it. Amongst this number are Todhunter, Hymer, Drew, Hamilton, Hustler, Bridge, Puckle, and Jackson.

In the Oxford, Cambridge, and Dublin Messenger of Mathematics, vol. III. page 151, R. Tucker, M.A., has given a solution of this problem. The construction of the magnitude of the axes is simple; their directions, however, are not so simple, as they are made to depend upon the focal properties of the ellipse.

This provoked another solution in the same volume, page 227, by R. A. Proctor, B.A., whose construction is very simple indeed. The lines employed by Mr. Proctor are different from those employed by Mr. Millar, but the construction of each is about equally simple.

In all the cases above referred to the construction of the principal axes only is given. The direct construction of any pair of conjugate diameters from the first pair is not considered by any of the above named authors. The following is offered as a solution of this part of the problem. I am not sure it is the simplest that could be devised. It depends upon property (7) when the axes are not the principal axes.

Let AOA' BOB' be the given conjugate diameters. Draw BtT parallel to AA'. With the centre B and radius OA draw the circle DHh. In this circle take any radius BD at pleasure, perpendicular to which draw the diameter HBh. Find O' in BD such that $O'H=O'O=O'h$, make $O't=O'T=OO'$, join Ot and OT. These lines will be conjugate. To find the length of the diameters draw Bm parallel to Ot, and Bn parallel to OT, then $OM^2=OT.Om$ and $ON^2=Ot.On$.

A simple construction with a circle is usually adopted to determine OM ON geometrically from these two equations.

Mr. Arthur Mc.Dougall, B.Sc., invited attention to a specimen of carbon formed upon the roof of a gas retort, by the decomposition of the hydrocarbon gas by heat. The carbon thus formed resembles graphite in its almost metallic lustre, and it was suggested that its mode of formation might throw some light upon that of graphite. Graphite always occurs in association with rocks which have been subjected to igneous action, and may have been formed by hydrocarbon gases traversing fissures or dykes whilst the sides were in a highly heated state, thus causing a deposit similar to that formed in gas retorts. The fact that in the latter case an increase of pressure causes a greatly increased amount of deposit favours this view, as it is extremely probable that any gases existing in the earth's crust would be in a state of great tension.

"On the Presence of Sulphate of Copper in Water heated in Tinned Copper Boilers," by WILLIAM THOMSON, F.C.S.

A few weeks ago I was consulted with regard to some water which was taken from a copper boiler in a kitchen range, which led me to investigate the case, and, as the results seem to be of vital importance to many, I venture to bring them before your notice.

The range referred to belonged to a large chapel in Manchester, and was employed for culinary purposes in connection with various meetings of the congregation, &c. It was originally formed of two iron boilers, each capable of holding from thirty to forty gallons, with a fireplace between. One of these boilers was cracked, through cold water having been carelessly thrown on the iron after it had been allowed to become nearly red hot. To repair this defect a copper boiler coated with tin was fitted into the cracked iron one. I was informed that this boiler together with the iron one on the other side of the range had been employed for heating water for tea making at one or more meetings, and that some persons complained of feeling ill after tea. Some of the hot water from this boiler was afterwards employed for washing, and as it broke up the soap like hard water and threw to the top a scum which had a bluish colour, suspicion was thrown on it and a sample of the water brought to me for examination. It contained some matter in suspension of a dark colour, which soon subsided and left the water clear. Presuming that if copper were present it would be in suspension and not in solution, I examined the sediment but found it to be free from that metal. I then filtered and examined the clear water.

It contained a large proportion of copper in solution, and gave a distinctly acid reaction to blue litmus paper. I evaporated the water down to a very small bulk and extracted the free acid with absolute alcohol, eliminated the alcohol used, and got it in a concentrated form in a water

solution, it charred paper with facility and gave a copious white precipitate with chloride of Barium insoluble in Hydrochloric acid, thus proving it to be free Sulphuric Acid which had acted upon the copper. I then continued my investigation to find the source of that acid. I observed that the boilers on each side of the fireplace were supplied by the same water, (Manchester supply) passing along the same pipe. I collected a sample of that water from the tap used for filling the copper boiler, and took another from the iron boiler on the other side, these samples were evaporated down and tested, but found to contain neither free acid nor copper. I next looked for the acid as a result of the combustion of sulphur in the coal, but as both boilers were entirely separated from the fire by brick work and also covered in on the top, the products of combustion had no chance of finding their way into the boilers, and further had this been the cause I ought to have found free acid in the water of the iron boiler.

On further enquiry, I was informed that after the boiler had been put in, it was well washed with water and afterwards had a solution of washing soda boiled in it and again washed well with clean water. After this treatment, water which was heated in it became highly contaminated with copper and free sulphuric acid.

My experiments up to this time having offered no solution of the problem as to how the water became contaminated, I made enquiries respecting the galvanizing of such utensils, and found that the process followed was this: the inside of the vessel was "pickled" with sulphuric acid, then rubbed with sand to remove oxide, and washed, lastly heated up with chloride of ammonium and block tin rubbed over the surface.

The only explanation which I can offer of this remarkable contamination is, that part of the sulphuric acid used for cleansing before galvanizing had been secreted in the joints

formed by the riveting of the sheets of copper together and also between the plates and the rivets, so that when the boiler was washed and heated with a solution of carbonate of soda, it could not enter into the crevices to neutralize the acid during the few hours the soda solution was heated, but which under the subsequent prolonged action of the hot water gradually dyalized out bringing the copper with it.

I called one day at the chapel, and found them using the hot water from this boiler for washing dishes, so that fresh quantities of pure water were being added as the hot water was drawn off. I took another sample of it, and on analysis I found it to contain 3·575 grains of metallic copper to the gallon, equal to 14·056 grains of crystallised sulphate of copper. I consider it well that results such as these should be generally known, as I understand that boilers of this kind are often employed for culinary purposes, and through ignorance of the above facts serious results might accrue.

Professor W. BOYD DAWKINS, F.R.S., exhibited a collection of articles of the Neolithic and Bronze ages from the pile dwellings in the Lake of Bienne, lately presented to the Manchester Museum, Owens College, by Joseph Thompson, Esq. He called attention to the fact that the neolithic peoples were the first herdsmen and farmers of whom we have any trace, and stated that to them we owe the introduction into Europe of domestic animals and of cultivated cereals. They were also the first weavers and gardeners. From the southern character of some of the domestic animals such as *Sus palustris*, and of some of the vegetables such as the Egyptian wheat and *Silene Cretica*, it may be inferred that they came from the south, probably from the south-east, from the warmer regions of Central Asia.

With regard to the Bronze age it is a disputed question as to whether the knowledge of bronze was spread by

commerce or by conquest. Probably it was spread by both these means. The *art* of the Bronze age can only be traced home to the Etruscans, that mysterious people who are a terror to the philologists, and of whom we know historically that they were powerful by land and sea, that they were famous workers in metal, and possessed of quantities of amber. He therefore thought it probable that the amber trade with the shores of the Baltic, and the tin trade with Spain and Britain, distributed over a large part of Europe, the produce of the Etruscan workshops. On the decay of the Etruscan power the trade was taken up by the Phœnicians, the great maritime people who possessed no distinctive style of art of their own, but manufactured goods for the various markets, like the manufacturers of Manchester and Birmingham. He did not therefore see how the popular view could be maintained that the art of the Bronze age was introduced into Northern and Central Europe by the Phœnicians.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 18th, 1875.

JOHN BARROW, Esq., in the Chair.

Mr. JAMES COSMO MELVILL, M.A., F.L.S., read a paper on the Botany of Wilmington, North Carolina, with an especial reference to the habitat of Dionæa muscipula (Ellis). He visited the place in May, 1872, and after passing an extensive sandy tract, N.W. of the city, where grew Robinia hispida, *var, Elliottii,* Lupinus diffusus, Stipulicida Setacea, &c., arrived at a pine barren, beyond which was a marsh, and found Dionæa growing amongst rushes, Helonias

dioica, Sarracenia purpurea, and S. flava—it appeared to be very local, even there. The soil seemed of the usual peaty formation common to the pine barren, and it is difficult to account for the plant having so restricted a range. The only insect found entrapped by the leaves was a species of midge. Another very local plant, the *Solidago verna*, grew close by, for which this is the only locality known.

Mr. MELVILL also exhibited a specimen of *Euptychia metableta* (Crosse), a land shell lately discovered in Madagascar, connecting the Genera Cyclostoma and Cyclophorus, and distinguishable from all other land shells by its scalariform variels.

February 15th, 1875.

CHARLES BAILEY, Esq., in the Chair.

Mr. ROGERS exhibited a specimen of *Carex ornithopoda*, Willd., collected by Mr. J. Whitehead in Millersdale, Derbyshire, in July of last year.

Mr. CHARLES BAILEY remarked that this specimen was identical with examples which he exhibited from Saxony and Lower Austria, and that it was a very interesting addition to the flora of the district as well as to that of Great Britain. Its nearest ally was *Carex digitata*, L., but this species is not known to occur in the neighbourhood of Manchester, although found in the extreme north of the county. Its distribution all over Europe is rather perplexing as it is entirely absent from extensive areas; it is rare in France and Luxembourg, and unrecorded for Belgium proper, Holland, and Denmark, It is a Scandinavian species, and common in the Rhine provinces, as well as in Germany, Austria,

Russia, &c. In some parts of the Continent the *Carex ornithopoda* is found associated with *Carex digitata,* but whether the former is a stunted, starved form of the latter, as Crépin surmises, or whether they are specifically distinct, are matters for further investigation. The intermixture of closely allied, but distinct, species is by no means infrequent, and their occurrence in immediate association with each other is not necessarily a proof of their common origin. Thus *Erythræa Centaurium* Pers., *E. littoralis,* Fries, and *E. pulchella,* Fries, are frequently found intermixed; *Medicago eu-falcata* and *M. sylvestris,* Fries, grow together on the Norfolk coast, and *Statice Behen,* Drejer, occurs side by side with *S. Bahusiensis,* Fries, in the estuary of the Wyre opposite Fleetwood. The botanist who studies the living plant and notes the habits and surroundings readily separates these and other allied species, and there is little difficulty in differentiating *Carex ornithopoda* from *C. digitata,* its shorter rhizome and cymose spikes being the most manifest characters for identifying it from the last named species. As it may occur in other localities in the district, Mr. Bailey mentioned the following as the more striking characters ot the two plants:

	Carex digitata, L.	*Carex ornithopoda,* Willd.
Bracts	Membranaceous	Shorter than those of *C. digitata.*
Female Spikes	Distant; erect; with large fruits	Condensed; curving outwards; lighter in colour, and shorter than in *C. digitata.*
Glumes of Fruits	Attaining the base of the beak	Not reaching the base of the beak.
Beak of Fruit	One-eleventh the length of the fruit	One-fifteenth the length of the fruit—which is a shade smaller than that of *C. digitata.*

A full account of the more minute differences between the two species will be found in Crépin's paper, in Vol. XVIII Mémoires couronnés et autres mémoires publiée par l'Académie de Belgique (1865).

Mr. SIDEBOTHAM, F.R.A.S., then read a paper entitled "Notes on the Botany and Natural History of Tenby and the neighbourhood." Specimens of some of the rarer plants found in that locality were exhibited, and critical remarks on the specific identity of some of them were made; also on some of the land shells, of which a good series was exhibited, with some interesting varieties, specimens of which were distributed among the members present.

Mr. SPENCER BICKHAM read an interesting paper on the different kinds of Beehive used in this country, and exhibited specimens.

Special General Meeting, March 23rd, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

The PRESIDENT said:—The subject which the meeting will have to consider this evening is one of considerable importance. We have met to discuss and come to a decision on a scheme first proposed by the Council and generally approved of by the Society at its annual meeting in April, 1873, a scheme for the incorporation of the Society under the provisions of the Companies Acts 1862 and 1867, and the adoption of a new code of laws which this scheme if carried out will render necessary. This Society, as you are aware, has hitherto occupied the position simply of a private association for the promotion of Literature and Science, having been guided by laws or rules, which have frequently been altered and revised so as to adapt them to altered circumstances. The Laws and Regulations governing the Society at the period of its establishment will be found in print at the commencement of the first volume of the Society's Memoirs, published in 1785. The two chief objects of the Society at that period (when the number of members was limited to fifty) seem to have been the reading of papers on various subjects at its meetings, and the subsequent publication of such of them as the "Committee of Papers" should approve of in its Memoirs, and the "Laws" refer principally to these objects. The secondary objects which the Society had in view, such as the formation of a library, the award of medals to persons of merit, &c., were set forth in the so-called "Regulations." These Laws (the Regulations having in 1790 been incorporated with them)

subsequently underwent slight modifications. In 1852 a set of Rules was adopted by the Society, and duly certified in pursuance of the Act of Parliament. In these Rules the "Committee of Papers" is not mentioned, the decision as regards the printing of papers, as well as the management and direction of all the affairs of the Society, "subject to such instructions as may be given from time to time by the Society," being entrusted to the Council. The property of the Society is declared to be "vested in Trustees upon trust for the ordinary members, who shall alone be interested in the property of the Society." During the following years much discussion arose regarding these rules, and the result was the adoption in 1861 of a new code, being that by which the Society has been governed from that time to the present. The set of rules now in force does not differ essentially from the preceding one. A new feature however appears in the shape of an appendix referring to the Sections, which were not in existence at the time the previous set of rules was drawn up.

Without undergoing any change in its character and objects, the Society's sphere of activity has during the last twenty years been considerably enlarged. In addition to the Memoirs which appear periodically it publishes Proceedings, giving an account of what occurs at each of its meetings, and the labours of the Secretaries and the Editor of the Society's publications are consequently more onerous than formerly. Our library is now one of considerable extent and importance, and requires constant attention on the part of the librarian. Several sections have been established for the reading and discussion of papers on subjects with which they are specially concerned. Two societies having no organic connection with ours make use of our rooms for their meetings under certain conditions, and it may be expected that others of a similar character will be admitted to the same privileges. Our property is now of considerable

value, and yet under the present regime we are unable to deal with it easily and expeditiously. Under these circumstances it has been considered advisable that the Society should endeavour to attain to a legally recognised position and status, together with greater liberty of action generally, opportunity for which is afforded by the Companies Act, 1867. At the annual meeting of the Society held on the 29th of April, 1873, it was accordingly resolved that the Council be instructed to take steps for procuring the incorporation of the Society under the provisions of the Companies Acts, and to apply to the Board of Trade for permission to omit the word "Limited" from the title of the incorporated Society." At the following annual meeting, held on the 21st April, 1874, the Council in their annual report stated that "steps had been taken for procuring the incorporation of the Society under the provisions of the Companies Acts, but a question having arisen whether it may not be necessary to alter the rules of the Society, it has been thought desirable to obtain counsel's opinion on this point before proceeding further in the matter." The Council having placed the matter in the hands of Mr. H. M. Ormerod, solicitor, the Rules of the Society have been by him, as a necessary preliminary to the application to the Board of Trade, submitted to the revision of counsel, and have been by him re-drawn in the form of the Memorandum and Articles of Association now before you. The wording alone of the rules has been altered, but the spirit has been preserved, and the two combined contain all that is essential in our present set of rules, modified only in accordance with the requirements of the law. The Memorandum and Articles of Association having been submitted to and approved by the Board of Trade, who have granted their licence for the registration of the Society, it now remains for the meeting on behalf of the Society to adopt or reject them as it thinks fit. If the meeting approves of them, after confirming its resolution

as to the formation of a company under the Act, the further steps to be taken with a view to registration will follow in due course.

The following resolutions were then proposed by Mr. H. M. ORMEROD, seconded by Professor BALFOUR STEWART, and carried unanimously:—

1. "That this Society be formed into a Limited Company under the 23rd section of the Companies Act, 1867, and by the name of The Manchester Literary and Philosophical Society, and that the Council of the Society be and they are hereby authorised to take such steps in that behalf as they may deem expedient."

2. "That the Memorandum and Articles of Association, draft of which has been approved by the Board of Trade, and is now laid before this Meeting, be and they are hereby approved as the Memorandum and Articles of Association of this Society, when so incorporated, and that the Council do take such steps as may be necessary for procuring the same to be duly executed and duly registered in conformity with The Companies Acts, 1862 and 1867."

Ordinary Meeting, March 23rd, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

The following letter from Mr. J. B. MILLAR, B.E., was read by Professor REYNOLDS:—

Will you kindly permit me to express, through you, my thanks to Mr. Rawson for the trouble he has taken in comparing the solution which I gave of the problem on the ellipse with those which have been given by the various authors on the subject?

The points on which I relied when venturing to bring the problem under the notice of your Society were, its practical importance; the simplicity of my construction; and the fact that the same solution had not before been published, or if it had, was very little known. On all these points Mr. Rawson has confirmed the correctness of my opinion.

The solutions referred to by Mr. Rawson may, from his point of view, be conveniently arranged into three classes: (1) those which are quite different from mine; (2) one which *might* have been the same had it been done in a different way from what it is; and (3) one by Waud which is the same with one exception. I have not yet been able to find the book containing this last solution, and cannot therefore say how nearly identical the two solutions may be; but as the generating circle is the most important part of my construction, seeing it determines both the directions and lengths of the axes, and as Mr. Rawson says the circle is not introduced in Waud's solution, I cannot but think the exception must be an important one.

It is scarcely necessary to observe that I laid no claim to having discovered a new geometrical principle, but I did think that by looking at the problem from a different point of view from most of the writers on Geometrical Conics I had succeeded in obtaining a solution which was at once simple to execute and easy to remember, and I submit the proper test by which to judge it is to take the compasses and work the problem by the several known methods, when the simplicity of my method as well as its points of difference from the others will become apparent.

"On Discoveries in a Cave at Thayingen, near Schaffhausen," by ARTHUR WM. WATERS, F.G.S.

Last meeting our attention was drawn by Professor Boyd Dawkins' interesting paper to the large number of settle-

ments which have been found in Switzerland belonging to the Neolithic and Bronze ages. On the other hand traces of earlier inhabitants have only been found as yet in Switzerland in two caves at Salève and Villeneuve, on the west side of the country, and in the cave at Thayingen at the extreme east. In each articles of human workmanship have been found in association with bones of animals.

The examination of this last cave, which is called the "kessler loch," or kettle cave, since the Gipsy tinkers had often used it, was begun in the early part of 1874. It is situated in the Jura limestone, a few miles from Schaffhausen, but since it is rather a hollow in the rock it scarcely deserves the name of a cave. A full description of the fauna found here will shortly be published, with the assistance of Professors Rüttimeyer and Fraas, and until this has appeared it is impossible to give any exact list; but probably the following though not complete will be found correct:—Ursus arctos, Canis lagopus, Gulo borealis, Elephas primigenius, Bos primigenius, Canis vulpes, Lepus variabilis, Arctomys marmotta, Cervus tarandus and perhaps Cervus elephas and Equus. This is very similar to the lists from the other caves, and the animals belong to the upper pleistocene group. Nor does the similarity end in Switzerland, for besides the same animals some of the harpoons and other weapons have an extraordinary resemblance to some from the caves of South France, and sufficient with those of some parts of Germany and South England to show that the same race of men had a wide range.

In the periods which were brought under our notice last meeting we saw that the inhabitants of these dwellings were herdsmen and farmers, who cultivated the land, had several domestic animals, followed the chase, erected houses, skilfully worked stones into implements and afterwards used bronze tools, made pottery, spun and wove flax and bast, with which they made clothes and nets; but when we

turn to these earlier cave men, whether in Switzerland or elsewhere in Europe, we find that the life they led was altogether different, for they seem to have been a race of hunters and fishers, with probably no domestic animals, no knowledge of how to cultivate land or to erect themselves dwellings, and the stone tools they used were never polished as in the later periods, but merely chips of flints. Yet these men whose civilization stood so low were able with their simple flint tools to execute engravings on bone and horn with fair skill. The main point of interest connected with the cave to which I am drawing your attention lies in the discovery among the remains from Thayingen of a piece of antler of reindeer with the representation of a reindeer feeding, most faithfully and really artistically carved. There are also two other carvings (of horses) of which no description has yet been published, but I have been told that the execution of one is even much superior to that of the reindeer.

From the paleolithic caves of the Dordogne and Switzerland numerous engravings of animals have been found, but none yet published are as well drawn as the reindeer from Thayingen; but Professor Heim, in the paper* from which I get much of my information regarding the cave, says that he has been informed that just recently M. Piette, juge de paix at Craonne (Aisne), has dug out caves near St. Bertrand, in the Pyrenees, which show the same conditions as those from Perigord in the Dordogne, and that a drawing of a wild goat still more artistic than our drawing from Thayingen has been found there.

From their implements and mode of life the palæolithic race of men is supposed to be represented by the Esquimaux at the present time, and this receives the strongest support from the fact that these people, whose mode of life is very

* Ueber einen Fund aus d. Renthierzeit in d. Schweiz von Prof. A. Heim. Mitt. Antiq. Gesell Zürich 1874. XVIII. 5.

similar to that of the palæolithic man, are in the habit of ornamenting their bone and horn implements in the same way, and the style of designs has much resemblance to that of the Dordogne.

From the cave near Schaffhausen harpoons, and the so-called commandostäbe, which have been shown by Professor Boyd Dawkins to be really arrow straighteners, together with needles or bodkins have been found. The drawing of the piece of antler, with the engraving, were lithographed by Professor Heim with the greatest care and exactness, and every slip of the flint is shown, and measurements of the depth of the lines by an instrument specially arranged for it are given in the work I have referred to. Prof. Heim also argues that the preponderance of animals looking to the left over those looking to the right indicate a probability that the artists drew with the right hand. He concludes by saying, "the race of zoo-artists were in their talents in advance of the means which were at their disposal. In the later races—for example the pile dwellers—the intellectual capacity and the resources in the midst of which the men grew up are more nearly balanced." He also says "that this was a premature attempt of the human genius, and that no partial inconsistent cultivation of a single talent can be maintained for a long period." This last remark does not seem to be borne out, since the similarity of the Esquimaux and paleolithic man is undoubted, and would rather make us consider how persistent a low civilization may remain when there are few extraneous modifying circumstances.

Ordinary Meeting, April 6th, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. J. S. Kipping and Mr. W. A. Cunningham were appointed Auditors of the Treasurer's accounts.

"A Study of Peat;" Part I. By R. ANGUS SMITH, Ph.D., F.R.S., &c., V.P.

The author referred to a former paper, in which he stated his belief that calculations as to the age of peat bogs were frequently much exaggerated, and that the more highly combustible bodies called hydrogen and richer hydrogen compounds increased in proportion to age, not by addition to their substance, but by the oxidation of the other parts, and removal of carbonaceous bodies in the brown water.

After referring to the observation of oil from peat and treatment of the subject and its relation to questions connected with coal, by E. W. Binney, F.R.S., he now wished to bring forward certain scientific and certain economic points. Of the former: 1st. The rapid growth of peat, shown partly by collected experience and by numerous quotations. 2nd. The existence of the resins and bodies having a high amount of hydrogen and carbon in new as well as old peats. 3rd. The cause of the rather greater amount in the old, not from formation during decay of the plant, but their greater permanence and insolubility. 4th. The existence of similar bodies in the fresh mosses which form the peat. 5th. The possible removal of all woody fibre by decomposition, leaving only oils and resinous or similar bodies undergoing little if any chemical change. 6th. The reason, viz., that woody fibre produced no highly

combustible liquids or solids during its decomposition. 7th. Suggesting that the same idea might be transferred to coal without confining it to any portion of the plant. 8th. The rapid formation of hard peat promoted by the growth of fine mosses, which break down readily into fine powder, whereas strong stems remain long.

Next, certain economic and sanitary points. 1st. The importance of keeping up in certain parts of the country a sufficient amount of peat for the fuel of the neighbourhood. 2nd. The possibility, by proper peat culture, of increasing the growth manifold. 3rd. The question of the removal of the resinous, &c., bodies by solution instead of distillation. 4th. The value of peat as a reservoir of water. 5th. Its value as a rapid grower, if properly cultivated, for filling up wet ground and swamps. 6th. Its subsequent use in removing swamp fever, which was never found, at least in the northern peat bogs, and he believed never in the true peat bog, the cold not being the cause of this.

On recent enquiry, he had found peat which, on good evidence, had grown 30 inches in 44 years, and observers with still better opportunities gave amounts much higher, equal to 88 inches in a similar time. Taking the lower estimate, it seemed clear that Sprengel's statement was correct, that peat grew more combustible matter in an acre than forest trees grew. The specimen in question, from Deeside, on a spur of the Grampians, had a considerable density, viz., 0·92, measuring externally, or a cubic foot weighed 57lbs., whereas a cubic foot of water weighs 62·32. The probably very old peat of a coaly fracture spoken of by several persons is not here alluded to, and did not come under the author's observation. The amount of the wet material that grew in an acre was 2,454 cubic feet in a year, and of the dry material, taking the lowest estimate of 1-6th, 10·2 tons, say ten. The estimated amount for wood in the plains below was considered to be 2½ tons per acre

per annum, and this may be high, as Liebig gives only half this amount, and hay and straw, which he considered able to produce the same amount of dry material, do not produce more. The value of the peat and the wood as combustibles would differ little. The value of the ten tons may be considered as equal to four tons of coal. These four tons, then, were grown on land which could scarcely be said to have agricultural value. Of course, all bogs are not always growing at this rate, and there is a limit in time to their increase; on the other hand, by care and proper feeding, we may be able to grow the material much faster than ever it has grown, and the black bogs may become for us rich coal fields, oil wells, and whale fisheries. The fuel is not important for those places where coal is cheap, but there are many places in Great Britain where coal is difficult of transport. Since peat will not bear much carriage, its value is limited in distance, and in many places now without any it would be well to grow along with oat and potato fields a field also of this despised fuel. This is actually done, but it is rarely systematic, and in many places peat is driven out where a portion of land otherwise almost useless might be set aside for it. The systematic fostering of peat so as to increase the produce is advocated for many places, although this may appear absurd in the eyes of those who are desirous of removing entirely all its traces.

The peat which had grown rapidly was fine in texture and became heavy on drying; not of the heaviest kind, as 0·92 is high but not of the highest class. (The lightest peat examined was 0·2.) The first had grown from fine or small mosses, *hypnum* chiefly, and the fineness of the fibre was apparently the cause of the rapid breaking up. It is not time alone then that is always required to make fine dense peat. Large pieces of wood may endure and be preserved long. Henceforth a new classification of peats is necessary, and this is according to the prevailing plant. To grow good

peat, all plants with thick stems must be avoided as giving too much woody fibre, rendering the structure too open and the peat too light, as well as giving an inferior amount (in the case of some woods at least) of the resinous and very inflammable bodies, and generally taking too long to form. The great peculiarity of good peat is the oleaginous and resinous matter, to which also wax and fats may be added, as they have been found by some. It has been generally believed that these bodies have been produced during the decomposition of the plant, although one writer, quoted in the paper, considers they were produced by the growing plant. Dr. Smith came to the conclusion that woody fibre could not be shown to produce substances rich in hydrogen, the compounds resulting from its decay were rather of a humous character and not good combustibles. If woody fibre did not leave its hydrogen and carbon in such quantity as to form the resins, &c., of peat, then we must look for the origin in the growing plant. The mosses from which the peat from Deeside was evidently grown were examined, and on drying gave about a fourth of dry matter which readily crumbled into a powder and which contained 1·27 per cent of substances soluble in a light naphtha. It much resembled that obtained from the peat, but was softer, being about the consistence of butter and capable of being distilled so as to give a yellowish substance of the same consistence. Besides this there was 1 per cent of a substance extracted by alcohol, resinous in appearance, fusible, and containing apparently the chlorophyll.

The author believed that these bodies produced the similar matter in the peat, or rather were the matter itself with little or no change. In the peat it had been hardened, perhaps by oxidation or perhaps by the removal of the more fluid portion by water. In this way he explained the possibility of having a flow of oil from a peat moss. When the substances in the plants themselves were of a more fluid

character, the removal of the woody fibre and absorbing humous bodies would set them free, and the fusibility or otherwise of the substances at ordinary temperatures would depend on the plants. The oil formed by distilling the resinous bodies obtained from the peat was of a light yellow. Its true character has not been fully determined, but the analysis gave—carbon 83·86 p.c., hydrogen 12·70.

The author inclined to believe that there is a great variety of oils and solid hydrocarbonaceous bodies, if not true hydrocarbons, in the plants, which so far as he knew had not been examined. The resins of the peat had been carefully examined by Mulder.

The author had not found any of the coaly peat spoken of by some authors. It might readily be supposed that when the woody fibre was removed various compounds insoluble in water would remain and account for fossil resins, ozokerit, &c. A similar action might produce coal although the plants forming peat and coal were different and most probably the climate, and the idea of Prof. Morris (see Prof. Huxley and Prof. Dawkins on coal) that the bituminous part of coal was composed of spores and sporangia primitively supplied with resinous or oleaginous matter would so far agree with the above reasoning, although in forming peat we cannot confine ourselves to the spores, so far as the author knew at least, when wood is present having resin dispersed in it. Perhaps the same may be said of coal.

The author spoke of the great value of peat as a water reservoir in a country demanding so much water, which did not always require to be bright, and of the possibility in many positions of clearing it as it was leaving the mosses. Water reservoirs could thus be grown at a cheap rate in many spots, instead of being banked in or dug at a great expense, although growing might require perhaps more time. A reservoir formed of peat 10 feet thick would hold as much as a water reservoir of the usual kind 8 feet deep, or say 7½, and still be easily walked over.

The capacity of growing possessed by peat was so great, if the results found can be readily attained in many places, that in suitable situations it might be used for filling up swamps and making dry surfaces, for after rising to a certain height the top easily drained and left a part dry.

Swamps were sources of fever and ague. True peat bogs never were, so far as the author knew, and probably the growing of peat would render many places healthy which could not otherwise easily be made so. Gases from peat mosses it is intended to examine more fully, and also many peat-forming plants.

PHYSICAL AND MATHEMATICAL SECTION.

Tuesday, March 2nd, 1875.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section in the Chair.

"Rainfall at Old Trafford, Manchester, in the year 1874," by G. V. VERNON, F.R.A.S., F.M.S.

The rainfall during 1874 was 34·095 inches and fell upon 203 days. The total rainfall was 1·617 inches below the average of the last 81 years, and 4·280 inches above the fall for 1873.

January had a rainfall above the average; February below, March considerably in excess; April, May, June, and July were all considerably below the average, June and July unusually so, the deficiency for these four months amounting to 5·540 inches below the 81 years' average; August and September were above the average, September slightly below the average; November and December were above the average, especially November, the fall in that month reaching nearly 5 inches. April was the driest month of the year.

The rainfall for the first quarter of the year was 1·248 inches above the average; for the second and third quarters the fall was 3·762 inches and 0·408 inches below the average; and the last quarter 1·305 above the average.

The dry weather of April, May, June, and July appears to have suited the grain crops, which were remarkably large, and were no doubt greatly favoured by the extraordinary weather during the two last weeks of April, the temperature of which were 9·7° and 7·0° above the average 25 years.

Rain-gauge 3 Feet above the Ground and 106 feet above the Level of the Sea.

Quarterly Periods. 1873.	Quarterly Periods. 1874.	1874.	Fall of Rain.	Average of 81 years.	Difference.	No. of Days' Rain-fall in 1874.	Quarterly Periods. 81 years.	Quarterly Periods. 1874.	Quarterly Periods. Diff.
			inches.	inches.	inches.		inches.	inches.	inches.
		January....	3·525	2·556	+0·969	18			
48	47	February.	1·589	2·376	—0·787	11	7·233	8·481	+1·248
		March.....	3·367	2·301	+1·066	18			
		April	0·999	2·030	—1·031	15			
43	40	May........	1·395	2·285	—0·890	17	7·156	3·394	—3·762
		June.......	1·000	2·841	—1·841	8			
		July........	1·771	3·549	—1·778	14			
64	55	August....	4·345	3·520	+0·825	20	10·383	9·975	—0·408
		Sept'mber	3·859	3·314	+0·545	21			
		October...	3·758	3·897	—0·139	25			
43	61	November	4·851	3·778	1·073	17	10·940	12·245	+1·305
		December	3·636	3·265	0·371	19			
198	203		34·095	35·712	—1·617	203	35·712	34·095	—1·617

"Results of Rain-gauge Observations, made at Eccles, near Manchester, during the year 1874," by Thomas Mackereth, F.R.A.S., F.M.S.

The rain fall of 1874, though reaching about the average for this district, is exceptional in several ways. The rainfall for the past three years has unusual characteristics, that of 1872 was remarkable for its excess, being one of the wettest years on record, that of 1873 was rather remarkable for its deficiency, and it may be said of the fall of last year that it was remarkable for both its deficiency and excess.

Though, as I have said, about the average amount for the year fell, there was a great deficiency throughout the spring and summer months as far as to the end of July. The thunderstorms which occurred in August seem not only to have been the precursor of the excesses of rainfall which followed, but also of a very rapid decline of temperature which continued with slight variations till it culminated in the very low temperature at the end of the year. The drought of April and May was accompanied by severe frosts that completely destroyed the fruit crops of this district at least. The number of days on which rain fell during the past year is greater than the average. But this excess is less in proportion to the amount of rainfall in the wet portion of the year than the deficiency in the dry portion, pointing again to the law which I showed to exist in the amount and frequency of rainfall, in a paper I read on the subject before the Section in the last session. The following table shows the results obtained from a rain-gauge with a 10in. round receiver placed 3ft. above the ground :—

Quarterly Periods		1874.	Fall in Inches.	Average of 14 Years.	Difference.	Quarterly Periods.	
Average of 14 years.	1874.					Average of 14 Years.	1874.
Days.	Days.						
52	56	January	3·261	2·813	+0·448	7·498	7·716
		February	1·464	2·194	—0·730		
		March	2·991	2·491	+0·500		
46	44	April	0·919	1·995	—1·076	6·686	3·764
		May	1·956	2·076	—0·120		
		June	0·889	2·615	—1·726		
53	60	July	2·136	3·051	—0·915	10·371	11·380
		August	5·767	3·280	+2·487		
		September ...	3·477	4·040	—0·563		
57	61	October.........	4·091	4·258	—0·167	10·529	12·371
		November ...	5·111	3·270	+1·841		
		December......	3·169	3·001	+0·168		
208	221		35·231	35·084	+0·147		

In the next table are given the results obtained from rain-gauges of two different kinds, placed in close proximity

in the same plane, and 3 feet from the ground. The one has a 10in. round receiver, and the other a 5in. square receiver. The large receiver had an excess over the small one in nearly all the drier months of the year. There were six months when the large one had the excess, and six months when the small one had it. The excesses in either case are exceedingly small, very few of them reach $\frac{1}{10}$ of an inch for the month. The greatest excesses are in the small gauge, and those in the wettest months of August, October, and November. There appears quite an unusual monthly excess in December in the small gauge. It attained to more than half an inch; but the conditions under which it happened are altogether abnormal. Most of it was melted snow. It fell heavily, fitfully, and often curiously intermixed with large and small flakes. This I offer as a suggestion as to the cause of the excessive difference in last December between the fall in the two gauges. An average fall of both gauges over a period of seven years shows only a little over $\frac{1}{10}$th of an inch of excess in the larger receiver; thus the two gauges may be taken as good checks upon each other.

1874.	Rainfall in inches in 10in. round receiver 3ft. from ground.	Rainfall in inches in 5in. square receiver 3ft. from ground.	Difference.	From 1868 to 1874. Average of 7 years' rainfall in inches in 10in. round receiver 3ft. from ground.	From 1868 to 1874. Average of 7 years' rainfall in inches in 5in. square receiver 3ft. from ground.	Difference.
January	3·261	3·219	+·042	3·026	3·011	+·015
February	1·464	1·440	+·024	2·137	2·099	+·038
March	2·991	3·011	—·020	2·406	2·441	—·035
April	0·919	0·863	+·056	2·008	1·977	+·031
May	1·956	1·903	+·053	1·913	1·876	+·037
June	0·889	0·872	+·017	2·355	2·317	+·038
July	2·136	2·096	+·040	2·793	2·776	+·017
August	5·767	5·971	—·204	3·272	3·251	+·021
September	3·477	3·538	—·061	3·866	3·821	+·045
October	4·091	4·192	—·101	4·976	4·968	+·008
November	5·111	5·225	—·114	3·154	3·190	—·036
December	3·169	3·708	—·539	3·288	3·350	—·062
	35·231	36·038	—·807	35·194	35·077	+·117

In the next table I give the results obtained from two exactly similar gauges, placed at different heights from the ground, and free from every interference. Each gauge has a 5in. square receiver, and the one is placed 3 feet and the other 34 feet above the ground. The total fall in the one 3 feet from the ground was 36·048 inches, and in the one 34 feet from the ground it was 28·201 inches for last year. The difference between the fall in the two gauges is 7·847 inches, or about 22 per cent less rain fell in the higher than in the lower gauge. In the same table I give the average fall in the same gauges for seven years, and by comparing the results it will be found that the average difference between the fall in the two gauges is about 18 per cent.

1874.	Rainfall in inches in 5 in. square receiver 3 ft. from ground. 1874.	Rainfall in inches in 5 in. square receiver 34 ft. from ground. 1874.	From 1868 to 1874. Average fall of rain in inches for 7 years in 5 in. square receiver 3 ft. from ground.	From 1868 to 1874. Average fall of rain in inches for 7 years in 5 in. square receiver 34 ft. from ground.
January	3·219	2·339	3·011	2·160
February	1·440	1·170	2·099	1·586
March	3·011	2·301	2·441	1·911
April	0·863	0·672	1·977	1·689
May	1·913	1·849	1·876	1·704
June	0·872	0·699	2·317	2·062
July	2·096	1·823	2·776	2·463
August	5·971	4·758	3·251	2·718
September	3·538	2·773	3·821	3·243
October	4·192	3·264	4·968	4·090
November	5·225	4·248	3·190	2·473
December	3·708	2·305	3·350	2·695
	36·048	28·201	35·077	28·794

The following table gives the ratios of the excesses of rainfall at 3 feet from the ground over the amount measured at 34 feet from the ground. I produced this kind of table for the first time last session, and I then showed that these ratios for the rainfall of 1873, and for the average of six years, were almost identical. The last year presents a very different result. Not only is the ratio of each month very divergent from the average, but the ratios of the totals are very different. I must, however, observe that both the

mean monthly and the mean annual values of six years, and the same means of seven years, have similar if not identical ratios. When, however, the mean monthly ratios are examined it will be found that the greatest difference of the fall happens in May. The six years' average places this difference in June. Before it can be seen to which of these months it really belongs, a larger area of averages is required. The fall of 1874 places it in May. Both the six and the seven years' average place the least difference in January, and this in a most marked manner. On the theory then, as I have pointed out before, that the excess of rainfall in the lower gauge is due to the particles of invisible vapour in the air between it and the higher gauge coalescing with the falling raindrops, the result seems to show that in the spring and early summer months there is relatively less of this vapour in the air below a height of 34 feet, and that there is relatively more of it in the winter months, and particularly in January. Hence the maximum of dry air on the ground is in May or June, and the minimum in January. The time of this maximum is also the time when the maximum of ozone is found.

Monthly and Annual Ratios of the Excess of Rainfall Measured at 3 Feet from the Ground over the Amount Measured at 34 Feet from the Ground.

	Ratios of such Rainfall for 1874.	Ratios of such Rainfall for an Average of 7 years from 1868 to 1874.
January	·726	·717
February	·812	·755
March	·764	·782
April	·778	·854
May	·966	·908
June	·801	·889
July	·869	·887
August	·796	·836
September	·783	·848
October	·778	·823
November	·813	·775
December	·621	·804
Annual ratios	·792	·823

In the next table I give the fall of rain for 1874 during the day from 8 a.m. to 8 p.m., and the fall during the night from 8 p.m. to 8 a.m. The results obtained from this table show another exceptional character of the rainfall of last year. Hitherto every year since I have made this comparison the total fall for the year in the day time has exceeded the total fall during the night. But during the past year the reverse has taken place and that to a large extent. In 1873 the total excess of the day over the night fall was 2·846 inches, or about 18 per cent, whilst during the last year almost the reverse happened, for the night fall exceeded the day fall by 2·692 inches, or about 15 per cent.

1874.	Rainfall in inches from 8 a.m. to 8p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Difference between Night and Day fall.
January	1·357	1·862	+0·505
February	0·619	0·821	+0·202
March	0·958	2·053	+1·095
April	0·367	0·496	+0·129
May	0·767	1·136	+0·369
June	0·602	0·270	—0·332
July	0·447	1·649	+1·202
August	3·114	2·857	—0·257
September	1·953	1·585	—0·368
October	2·119	2·073	—0·046
November	2·668	2·557	—0·111
December	1·702	2·006	+0·304
	16·673	19·365	+2·692

In the next table I present the average day and night fall for a period of seven years. The results of this table continue, notwithstanding the reversal of last year, to confirm the experience of previous years, that the day fall exceeds the night fall as far as the amounts of the whole year are concerned. The same curious fact presents itself in this table which is presented in a similar table of the previous two years. It is this: that an average excess of night rainfall occurs in January, February, August, September, November, and December. The total excess of the fall

during the day is very slight, though some of the monthly differences are very large, being in some cases as much as 100 per cent.

AVERAGE OF SEVEN YEARS, FROM 1868 TO 1874.

1874.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Difference between Night and Dayfall.
January	1·326	1·684	+0·358
February	0·876	1·222	+0·346
March	1·316	1·125	—0·191
April	1·148	0·828	—0·320
May	1·143	0·733	—0·410
June	1·326	0·990	—0·336
July	1·532	1·244	—0·288
August	1·602	1·648	+0·046
September	1·799	2·021	+0·222
October	2·560	2·407	—0·153
November	1·553	1·637	+0·084
December	1·484	1·866	+0·382
	17·665	17·405	—0·260

MICROSCOPICAL AND NATURAL HISTORY SECTION.

Monday, March 15th, 1875.

Professor W. BOYD DAWKINS, F.R.S., in the Chair.

The Honourable J. Leicester Warren, M.A., was unanimously elected an Associate, and Mr. Arthur William Waters, F.G.S., a Member, of the Society.

Mr. CHARLES BAILEY exhibited a series of slides illustrating the structure of Natural and Artificial Cork.

Mr. JAMES COSMO MELVILL exhibited a specimen of Coquina Stone from the quarries of Anastatia Island, St. Augustine, East Florida. This specimen was taken from

the castle of St. Mark, the earliest recorded building in the United States, 1565 A.D., which is entirely built of Coquina; the shells found in it appear to be chiefly those of *Arca incongrua* and *Ostrea virginica*, both species still living in the Western Atlantic.

Mr. T. S. PEACE exhibited a series of curious varieties of *Helix nemoralis, H. hortensis*, and *H. hybrida*.

Dr. ALCOCK showed a Greenland Bullhead (Cottus *Grœnlandicus*, which was taken in November, 1872, amongst stones at low water on the shore of the Menai Straits. He stated that, so far as he knew, it is the first example met with on the coasts of Britain, the two specimens previously recorded, and noticed by Couch, having been captured in Dingle Harbour, County of Kerry, in Ireland, in the month of November, 1850.

Annual Meeting, April 20th, 1874.

Dr. Schunck, F.R.S., F.C.S., President, in the Chair.

The following Report of the Council was read by one of the Secretaries:—

The Council have the satisfaction to report that the proceedings taken for procuring the Incorporation of the Society under the "Companies Acts, 1862 and 1867," in accordance with the resolution passed at the Annual Meeting held on the 13th of April, 1873, have been completed, and that a Certificate of Registration of the Incorporation of the Society was granted by the Registrar of Joint Stock Companies on the 22nd of March, 1875. Copies of the Memorandum and Articles of Association have been printed for distribution among the members of the Society.

The Treasurer's account shows that the expenditure during the year ending March 31st, 1875, was £57 3s. 10d. in excess of the income. This excess, however, is attributable to extra expenses for providing new book-shelving, rearranging the library, binding a large number of books, and preparing a new catalogue.

The number of ordinary members on the roll of the Society on the 1st of April, 1874, was 171, and 8 new members have since been elected; the losses are—deaths, 4; resignations, 5; and defaulters, 2. The number on the roll on the 1st of April instant was, therefore, 168. The deceased members are Thomas Standring, Sir William Fairbairn, Bart., T. T. Wilkinson, and Robert Hyde Greg.

Sir William Fairbairn, Bart., F.R.S., LL.D., who died on the 18th of August last, was born at Coldstream, near Kelso, on the 19th of February, 1789. At the age of 16, his father being then engaged in managing the Percy Main Colliery Company's farm, in the neighbourhood of Newcastle-on-Tyne, he was articled as an engineer for five years to the owners of the Percy Main. Thus began a career which, while protracted to an unusual length, has certainly been as remarkable for success and usefulness as any which this remarkable age has furnished. As several very complete biographical notices of Sir William Fairbairn have already appeared, particularly those written by Mr. Smiles and Sir Thomas Fairbairn, it is unnecessary here to enter into the incidents of his life.

After travelling as a journeyman mechanic for nearly four years, during which time he worked in London, in the south of England, and in Dublin, he settled in Manchester as a working millwright in 1814. In 1817 he and James Lillie commenced business on their own account, and having obtained employment first of all from Mr. Adam Murray, soon distinguished themselves by the attention and ability with which they conducted their business.

At this time Fairbairn was instrumental in effecting a revolution in mill machinery—in the means of communicating power. Hitherto engineers were accustomed to aim at slow motion, and this necessitated the use of very heavy shafts and large drums, which were for the most part made of wood or cast iron. Fairbairn, with true instinct, saw that by increasing the speed of the shafts he might reduce their size and employ wrought iron instead of cast iron or wood. He was enabled to carry out his improvements in a new mill belonging to Mr. John Kennedy, and their success completely established the reputation of Fairbairn as an engineer. He continued to introduce improvements in mill

machinery, particularly in the construction and working of water wheels; and so early as 1830 his opinion seems to have been sought far and wide.

In 1829 he undertook some experiments on the traction of boats on the Ardrossan Canal, which led to his constructing in Manchester an iron vessel, which appears to have been the third sea going vessel ever constructed of iron. The success of this vessel induced him to commence iron ship building, and in 1835 he opened a yard at Millwall, which was one of the first iron shipbuilding yards in the world, and from which has been turned out the largest ship yet produced, viz., the Great Eastern, constructed by Mr. Scott Russell, who subsequently purchased the yard from Mr. Fairbairn.

Although Sir William Fairbairn early took an interest in many scientific enquiries and has published many papers his by far most important work has been the extension of the use of iron as a material for construction. He early looked upon it from a scientific point of view, and commenced making systematic experiments. In 1837 he presented the British Association with a report "On the strength and properties of cast iron from the hot and cold blasts;" and he subsequently co-operated with Mr. Eaton Hodgkinson in a series of experiments on the strength of iron, which were made at Fairbairn's works. These experiments were on a scale such as had never before been attempted. They were conducted with extraordinary practical skill, and owing to the high scientific attainments of Mr. Hodgkinson, were so thorough and complete that in many respects they have left little to be desired, and are still the principal data which engineers rely upon.

Perhaps the work for which Sir William Fairbairn is best known, and which sprang directly out of his experience in shipbuilding and in the use of iron, was the assistance which, in connection with Mr. Hodgkinson, he rendered to

Robert Stephenson in the designing and construction of the Britannia tube.

Although it has been the subject of an unfortunate dispute to whom the merit of this great work belongs, yet owing partly to the researches of Mr. Smiles, the country knows enough to insist on assigning to all three a full share of merit, for it is clear that each of them performed his work in a perfect manner. Sir William Fairbairn claimed to have originated the idea of a cellular structure for the tube, and he from the first advocated the plan of a simple tube without chains, which plan was eventually adopted with such success.*

Judged, however, by the light of subsequent experience, this plan, although it has in every respect fulfilled the most sanguine hopes, is not better than certain others; and it appears that recently but few tubular bridges have been constructed, while of the plans which have been adopted in place of it, some much more nearly approach to Stephenson's original idea of a tube supported by chains or a stiffened suspension bridge. Thus the fact that Sir William Fairbairn succeeded in persuading Stephenson to adopt his plan, must be looked upon as an instance of the extraordinary force of character and readiness of mind to which as well as to his ability Sir William Fairbairn owed his success.

Although not now much used for bridges, the cellular method is largely used in ship building. The Great Eastern is built on this plan, as are almost all our ships of war.

In 1862 Sir William Fairbairn conducted, in conjunction with Mr. T. Tate, a series of experiments on the density of saturated steam, which form a very valuable addition to our knowledge of that subject.

Sir William Fairbairn was elected a member of this Society in 1824, and was president from 1855 until 1860. In 1860

* Smiles' "Lives of the Engineers," Vol. III., note on p. 474.

he received a royal medal of the Royal Society for his scientific services; and at the meeting held in Manchester in 1861 he acted as president of the British Association. He was the recipient of numberless honours both English and foreign; and in 1869 her Majesty created him baronet in acknowledgment of his scientific services.

Besides the numerous papers which he contributed to the memoirs of this and other societies he published several larger works well known to engineers. Until the very last he took an active interest in scientific questions and in promoting scientific education. He was one of the founders of the chair of Engineering at Owens College.

The author of this notice who had the privilege of knowing Sir William Fairbairn during the later years of his life, desires to bear witness to the constant gentleness, geniality, and kindliness of this distinguished man, who was at all times ready to interest himself in whatever interested those about him.

Mr. Robert Hyde Greg, who died on the 21st of February last, at Norcliffe Hall, was born in September, 1795, in King-street, Manchester. He completed his education in Edinburgh, where he made the acquaintance of Sir Walter Scott. On leaving college he travelled in Spain, Italy, and the East, and subsequently published some critical notes on his travels. He appears to have been at Madrid dining with Sir Henry Wellesley when the news came of the Duke of Wellington's victory at Waterloo. In early life Mr. Greg took a leading part in politics, and was elected for Manchester in 1839. He was one of the founders of the Manchester Mechanics' Institution, and was a member of the Geological Society.

He was elected a member of this Society in 1817, and he contributed several papers on literary subjects, amongst

which is an important paper "On the Site of Troy and the Trojan Plain," published in the Memoirs for 1824, and another "On the Round Towers of Ireland," published in the same volume.

On account of his advanced years, Mr. Greg had for some time retired from public life, and had devoted himself to agricultural pursuits, and the cultivation of his estates at Norcliffe, and at Coles Park, Herts.

Mr. Greg was a zealous horticulturist, and his grounds at Norcliffe have long been celebrated for their extensive collection of trees and shrubs, many of which are of great interest. His especial favourites were the coniferæ and rhododendrons, and amongst the former are individual specimens which are unequalled in this neighbourhood for their age and dimensions, some of them having been planted nearly 40 years ago.

Acting upon the authority given by a resolution of the Society, passed at the last annual meeting, the Council have granted permission to the Scientific Students' Association to hold their meetings and place their library in the Society's buildings on condition of their paying £1 a meeting to the Treasurer of the Society, and £5 a session for the services of the attendant, Mr. Roscoe.

At a Council Meeting, held December 15th, 1874, Mr. R. D. Darbishire brought forward a request from the "Association for the promotion of the Education of Women," that the Society would allow them the use of the meeting room on the condition of their paying for it, when it was resolved "That the 'Association for the promotion of the Education of Women' be allowed the use of the meeting room of the Society on condition of their paying £1 a meeting, with suitable remuneration to the attendant."

The following papers and communications have been read at the ordinary and sectional meetings of the Society during the present session:—

October 6th, 1874.—"On the Ossiferous Deposit at Windy Knoll, near Castleton," by Rooke Pennington, Esq., LL.B.

"On some teeth from a fissure in Waterhouses Quarry, in Staffordshire," by Rooke Pennington, Esq., LL.B.

"On the Animals found in the Windy Knoll Fissure," by Professor W. Boyd Dawkins, F.R.S.

"On the Extent and Action of the Heating Surface for Steam Boilers," by Professor Osborne Reynolds, M.A.

October 20th, 1874.—"On Two Remarkable Pieces of Iron Furnace Cinder, containing Crystals of Fayalite," by William H. Johnson, B.Sc.

"On a Specimen of Stigmaria, having the Medulla perfectly preserved," by E. W. Binney, F.R.S., F.G.S., V.P.

"On the Conditions under which Palæolithic Implements are found in River-Strata and in Caves in associatien with the extinct Mammalia," by Professor W. Boyd Dawkins, F.R.S.

"On a Colorimetric Method of Determining Iron in Waters," by Mr. Thomas Carnelley, B.Sc., communicated by Professor H. E. Roscoe, F.R.S.

November 3rd, 1874.—"On the Corrosion of Leaden Hot-Water Cisterns," by Professor H. E. Roscoe, F.R.S., &c.

"On an Improvement of the Bunsen Burner for Spectrum Analysis," by Mr. F. Kingdon, Assistant in the Physical Laboratory, Owens College.

"Some Notes on Pasigraphy," by Henry H. Howorth, Esq., F.S.A

"On the Existence of a Lunar Atmosphere," by David Winstanley, Esq.

November 17th, 1874.—"Some Remarks on Dalton's First Table of Atomic Weights," by Professor H. E. Roscoe, F.R.S.

"Action of Light on certain Vanadium Compounds," by Mr. James Gibbons. Communicated by Professor H. E. Roscoe, F.R.S.

"On Basic Calcium Chloride," by Harry Grimshaw, F.C.S.

"On the Structure of Stigmaria," by Professor W. C. Williamson, F.R.S.

December 1st, 1874.—"Some Doubts in regard to the Law of Diffusion of Gases, by Henry H. Howorth, Esq.

December 7th, 1874.—"On Œcophara Woodiella," by Joseph Sidebotham, F.R.A.S.

December 15th, 1874.—"Some Particulars respecting the Negro of the Neighbourhood of the Congo, West Africa," by Watson Smith, F.C.S.

"Analysis of One of the Trefriw Mineral Waters," by Thomas Carnelley, B.Sc. Communicated by Professor H. E. Roscoe, F.R.S., &c.

December 29th, 1874.—"On a Case of Reversed Chemical Action," by James Bottomley, B.Sc.

January 12th, 1875.—"On the Sewerage of Towns and the Pollution of Rivers and Streams," by E. W. Binney, F.R.S., V.P.

"On the Action of Rain to Calm the Sea," by Professor Osborne Reynolds, M.A.

"On the Stone Mining Tools from Alderley Edge," by Professor W. Boyd Dawkins, F.R.S.

"Archaic Iron Mining Tools from Lead Mines near Castleton," by Rooke Pennington, LL.B.

January 18th, 1875.—"On the Botany of Wilmington, North Carolina," by James Cosmo Melvill, M.A., F.L.S.

January 26th, 1875.—"A Descent into Elden Hole, Derbyshire," by Rooke Pennington, LL.B.

"Certain Lines observed in Snow Crystals," by Arthur Wm. Waters, F.G.S.

February 2nd, 1875.—"Results of Meteorological Observations taken at Langdale, Dimbula, Ceylon, in the year 1873," by Edward Heelis, Esq. Communicated by Joseph Baxendell, F.R.A.S.

February 9th, 1875.—"A Method of Finding the Axes of an Ellipse when two Conjugate Diameters are given," by J. B. Millar, .E. Communicated by Professor O. Reynolds, M.A.

"Note on the late Mr. James Wolfenden, of Hollinwood," by E. W. Binney, F.R.S., V.P.

February 15*th*, 1875.—"On Carex ornithopoda," by Charles Bailey, Esq.

"Notes on the Botany and Natural History of Tenby and the neighbourhood," by Joseph Sidebotham, F.R.A.S.

February 23*rd*, 1875.—"On some specimens of a strong arenaceous shale containing numbers of macrospores of Lepidodendron," by E. W. Binney, F.R.S., V.P.

March 2*nd*, 1875.—"On the Rainfall at Old Trafford, Manchester, during the Year 1874," by G. V. Vernon, F.R.A.S., F.M.S.

"Results of Rain-Gauge Observations made at Eccles, near Manchester, during the Year 1874," by Thomas Mackereth, F.R.A.S., F.M.S.

March 9*th*, 1875.—"On Mr. Millar's Method of Finding the Axes of an Ellipse when two Conjugate Diameters are given," by Robert Rawson, Esq., Honorary Member of the Society.

"On a Specimen of Carbon resembling Graphite formed upon the roof of a Gas Retort," by Arthur McDougall, B.Sc.

"On the Presence of Sulphate of Copper in Water heated in Tinned Copper Boilers," by William Thomson, F.C.S.

"On a Collection of Stone and Bronze Articles from the Pile Dwellings in the Lake of Bienne," by Professor W. Boyd Dawkins, F.R.S.

March 23*rd*, 1875.—"On Discoveries in a Cave at Thayingen, near Schaffhausen," by Arthur Wm. Waters, F.G.S.

March 30*th*, 1875.—"Photography as applied to Eclipse Observations," by Alfred Brothers, F.R.A.S.

April 6*th*, 1875.—"A Study of Peat, Part I.," by R. Angus Smith, Ph.D., F.R.S., V.P.

Several of the above papers have already been printed for the forthcoming volume of the Society's Memoirs, and others have been passed for printing. It is expected that the printing of the new volume will be completed before the commencement of the ensuing session.

The Council consider it desirable to continue the system of electing Sectional Associates, and a resolution on the subject will be submitted at the annual meeting for the approval of the members.

The Librarian reports that he has forwarded to the various societies with which we correspond Vols. 8, 9, 10, 11, 12 of our Proceedings and Vol. 4 of Memoirs. The Library has been re-arranged; a large number of volumes have been bound; additional shelves have been added; the catalogue has been completed, and will shortly be ready for the printer. The number of societies with which we correspond continues nearly the same as last year.

THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

DR. SAMUEL BROUGHTON, TREASURER, IN ACCOUNT WITH THE SOCIETY FROM APRIL 1ST, 1874, TO MARCH 31ST, 1875. CR.

		£ s. d.	£ s. d.	£ s. d.
1874. April 1.—To	[illegible]			590 4 11
"	[illegible]	361 4 0		
	.. 4, .. 3, ..15 } 22 at do.	46 4 0		
			315 0 0	
"	Arrears Received 4 [illegible]		9 9 0	
"	5 Members Elected in 1873, to Dec., at £2 2s.	10 10 0		
"	3 " " 1874, to Mar., at £1 1s.	3 3 0		
		13 13 0		
	Less 2 in Arrears, at £2 2s.	4 4 0		
			9 9 0	
"	8 Admission Fees, at £2 2s.	16 16 0		
	Less 2 in Arrears, at £2 2s.	4 4 0		
			12 12 0	
"	[illegible]		2 2 0	
"	[illegible] Section, at 10s.		1 10 0	
"	[illegible]	1 10 0		
	[illegible]	0 17 0		
			2 7 0	
"	*Sundry Income:* Sectional Contributions— Microscopical Section	2 2 0		
	Physical "	2 2 0		
			4 4 0	
"	[illegible]		14 19 11	
"	[illegible] rent, to March, 1874		7 10 0	
"	[illegible] year's rent, to Christmas		11 10 0	
				300 12 11
"	Interest on £1500 from the Owens College for Natural History Society, to December 25, 1874—Less Income Tax			59 8 9
				£1,049 6 7

General Balance:

	£ s. d.	£ s. d.
Compound Fund		98 15 0
Natural History Fund, last year	64 16 10	
" " " this year	59 8 9	
	£124 5 7	
Less Voted by Council for Purchase of Books	100 0 0	
		24 5 7
General Balance		419 0 6
		£542 1 1

		£ s. d.	£ s. d.	£ s. d.
1875. March 31.—By	[illegible] ty:			
	[illegible]	12 15 10		
	[illegible]	7 0 0		
	[illegible]	5 13 4		
				25 9 2
"	[illegible]			
	[illegible]	2 2 6		
	[illegible]	21 0 3		
	[illegible]	6 4 2		
	[illegible]	17 15 1		
				47 2 0
"	[illegible]	57 4 0		
	[illegible] Societies	5 9 0		
	[illegible]	22 0 0		
	[illegible]	11 0 1		
				96 13 1
"	*Publishing:*			
	Memoirs, Printing, and Engravings	9 12 6		
	Proceedings Printing	50 14 6		
	Editor of Memoirs and Proceedings	50 0 0		
				110 7 0
"	*Library:*			
	Periodicals	24 18 5		
	Dr Kestenberg—Assistance	4 0 0		
	Voted by Council for Binding, Shelves, &c. 100 0 0			
	Expended: Binding 50 10 1; Shelves 18 4 9; Assistant 30 0 0			
	98 14 10	98 14 10		
	Balance Unexpended 1 5 2			
	Subscription to Ray Society	1 1 0		
				129 14 3
				407 5 6
"	Natural History Section—Voted by Council for Purchase of Books			100 0 0
	Balance in Manchester Bank	496 5 6		
	" Treasurer's Hands	45 15 7		
				542 1 1
				£1,049 6 7

12th April, 1875.—Audited and found correct,
W. P. CUNNINGHAM.
J. O. KIPPING.

On the motion of Dr. Roscoe, seconded by Mr. Cunningham, the Report was unanimously adopted.

On the motion of Dr. Roscoe, seconded by Mr. Binney, it was resolved unanimously:—

"That the system of electing Sectional Associates be continued during the ensuing Session."

The following gentlemen were elected officers of the Society and Members of the Council for the ensuing year:—

President.

EDWARD SCHUNCK, Ph.D., F.R.S., F.C.S.

Vice-Presidents.

JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., F.C.S.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
ROBERT ANGUS SMITH, Ph.D., F.R.S., F.C.S.
Rev. WILLIAM GASKELL, M.A.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.
OSBORNE REYNOLDS, M.A.

Treasurer.

SAMUEL BROUGHTON.

Librarian.

FRANCIS NICHOLSON, F.Z.S.

Of the Council.

CHARLES BAILEY.
ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.
HENRY ENFIELD ROSCOE, B.A., Ph.D., F.R.S., F.C.S.
ALFRED BROTHERS, F.R.A.S.
BALFOUR STEWART, LL.D., F.R.S.

The reading of the paper entitled "A Study of Peat," 1st Part, by R. Angus Smith, Ph.D., F.R.S., V.P., was concluded.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 30th, 1875.

ALFRED BROTHERS, F.R.A.S., President of the Section, in the Chair.

The following gentlemen were elected officers of the Section for the ensuing year:—

President.
E. W. BINNEY, F.R.S., F.G.S.

Vice-Presidents.
ALFRED BROTHERS, F.R.A.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.
SAMUEL BROUGHTON.

Secretary.
G. V. VERNON, F.R.A.S., F.M.S.

"Photography as applied to Eclipse Observations," by ALFRED BROTHERS, F.R.A.S.

Since 1860, when photography was first applied to eclipse observations, almost every eclipse of the sun has been photographically recorded—from 1860 to 1868 for the purpose chiefly of determining the nature of the red prominences; and in 1870 and 1871 to ascertain whether the corona is an appendage of the sun or an effect produced in our own atmosphere. Previous to 1870 the ordinary telescope, uncorrected for the chemical rays, had been almost exclusively used. But in 1870 it was determined to adopt a properly corrected photographic lens, and by a graduated series of exposures to obtain if possible the whole pictorial effect. This method was successful, and has been adopted in all eclipse work since. That more suitable apparatus has not been employed may be due to the fact that the funds pro-

vided by the Government, the Royal and Royal Astronomical Societies, for the observation of the various eclipses have either all been spent at the time, or the balances have been returned. As good work has been done with the apparatus referred to, it may be asked why anything different should be used. It was by mere accident that a lens of a certain kind was used in 1870, no other suitable was to be had, and the image obtained with it is small. Photography was not employed during the eclipse of 1874, almost the only observer on that occasion being the Astronomer Royal at the Cape of Good Hope, Mr. Stone, who observed with the spectroscope under the most favourable conditions, and it is much to be regretted that no photographs were obtained. On the occasion of the recent eclipse no preparations were made until the invitation from the King of Siam was received, and then, as on almost every occasion since 1868, all arrangements have been hurriedly made. No apparatus for obtaining a picture of the corona different from what has been previously used was employed, and consequently no superior result may be anticipated. The lenses used in 1870 and since for photographing the corona give an image of the sun of about $\frac{3}{10}$ths of an inch in diameter, and although suitable for small pictures, such lenses cannot be said to be the best for the purpose. I would suggest therefore that at least three achromatic lenses of 5 or 6 feet focal length, corrected for the actinic rays, should be constructed, with all suitable apparatus, so as to be ready for use when required. The light of the corona is sufficiently actinic to produce good pictures when an instrument of long focus is used—it is only a question of time in the exposure and accuracy in the adjustment of the driving clock apparatus attached to the equatorial mounting. There cannot I think be any doubt that under favourable atmospheric conditions some features of interest would be revealed during every eclipse, and it is undesirable to allow any eclipse of the sun to occur without

some attempt being made to record such phenomena permanently by means of photography. It seems to me also equally certain that pictures of greater dimensions such as the instrument suggested would give would be proportionally more valuable than any hitherto obtained.

The photographic process used has always been the wet collodion. It might be advantageous to use daguerreotype plates, but I see no reason why both methods should not be employed.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

April 12th, 1875.

JOSEPH BAXENDELL, F.R.A.S., in the Chair.

Mr. ARTHUR W. WATERS, F.G.S., presented the section (for the cabinet) with the following slides:—

Diatomaceous earth from Billin, in Bohemia, full of Gallionella distans. Ehr.

Diatomaceous earth from Berlin.

Spicules of Alcyonium digitatum, from Brighton.

Dr. ALCOCK read a paper on the occurrence of Hyperia Galba, and Lestrigonus Kinahani, crustaceans parasitic upon Medusæ, at Southport, on a sand bank called the "Seldom seen" Bank.

A paper was read on the Mollusca, &c., inhabiting Cymmeran Bay, Anglesey, by Mr. JOHN PLANT, F.G.S., &c.

Cymmeran Bay is a deep indent on the western coast of Anglesey. At the head of the bay is the strait which

separates the small island of Holyhead. The bay at the widest part is five miles wide, from the small islet of Meibion, south, to the Rhoscolyn beacon islet, north. The head of the bay is an immense sand, over which a small stream finds its way to the sea. The north and south shores are rocky crags, and long islets of jagged and rough rocks occur at intervals in the bay, making it a source of danger to any vessels that drift from the channel, or in a fog lose their course and sail with the strong western winds right into the bay.

In places where the high cliffs have been worn away the shore is shallow and has miles of firm sand, whilst these sands are fringed with sand dunes, which inland stretch away in barren and wild commons with small bosses of rocks. The district is rarely visited by strangers, and altogether is a wild, stormy, and mainly uncultivated district. The rocks are grand for geological study; they are black slates, shales, serpentines, quartzites, granites, and conglomerates of Cambrian age. The last rocks are mines of the most valuable knowledge for the study of primitive rocks of pre-Cambrian age.

The sandy shores are extensive at the head of the bay; with continuous winds from the west and south the breakers are fearfully heavy, and the sea so rough that no boat from the shore can be launched by the fishermen for many weeks together; calms are rare and uncertain, so that fishing is precarious; at low tides a fine expanse of shore is exposed, as dry and as level as a bowling green, with the bosses of black and bright green rocks striking out of the level.

The conchologist would be greatly disappointed if he paid but a brief visit to the shores. He might walk all day and conclude that the sand was extensive but as barren of shells as a snowdrift. But with leisure and perseverance parts of the shore are found to be teeming with some species, and in the rocky pools and quiet caverns and corners many rare

specimens are to be found. At present my list of species of shells embraces eighty, but the list will be extended this year, when closer attention is paid to the specimens brought up by the dredgers.

The list of fishes comprises all that I have identified; no doubt there are many others caught in the bay.

SOME OF THE FISHES CAUGHT IN CYMMERAN BAY.

1. Spinax acanthias, spined Dog fish, caught during the herring season and at times amongst the rocks.
2. Raia clavata, Thornback Ray, not uncommon, is caught to cut up for baiting the Lobster pots.
3. Aculeatus marinas, Fifteen-spined Stickleback, in the rocky pools.
4. Latrax Lupus, Bass, caught in great numbers during summer, about the rocks; they are good eating.
5. Sparus aurata, common Sea Bream.
6. Trigla lyra, Piper Gurnard, occasionally caught out in the bay.
7. Scomber vulgaris, Mackerel, the fishing trawlers meet with shoals out in the channel, opposite the bay.
8. Gobius niger, Rock Goby, rare.
9. Labrus tinea, Ballan Wrass, found about the rocks in the bay.
10. Morrhua vulgaris, the Cod, the commonest fish in the bay.
11. Merlangus pollachias, Pollack Whiting, common during the summer.
12. Merlangus carbonarius, Coalfish, common along all the coast.
13. Ammodytes tobianus, Sand Launce, rather scarce.
14. Platessa vulgaris, the Plaice, uncommon in the bay.
15. Platessa limanda, the Dab, abundant in summer.
16. Solea vulgaris, the Sole, rare.
17. Clupea Harengus, the Herring, found passing the bay out in the channel.
18. Salmo salar, the Salmon. The salmon is caught in numbers in calm warm weather in summer, in the bay; it also runs up the river Crigyll to spawn, and is found in Lake Maelog.
19. Murœna conger, Conger Eel, caught with bait in the bay.

20. Sygnathus, Pipe fish, sometimes cast on shore.
21. Petromyzon marinus, Sea Lamprey, caught in the pools.

Hydrozoa. Pulmonigrada. Rhizostoma Cuvieri, large mushroom-shaped jelly fish, 14 inches diameter.

Physalia pelagica, Portuguese Man-of-war.

Sea Anemone, common smooth species, "Actinia mesembryanthemum."

SHELLS. CLASS I.—ACEPHALA.

Fam. IV.—Corbulidæ.

1. Corbula nucleus (2 specimens), rare.

Fam. VII.—Solenidæ.

2. Solen siliqua, rare.

Fam. IX.—Tellinidæ.

3. Psammobia Ferroensis, very rare.
4. Tellina crassa, common.
5. „ solidula, common.
6. „ fabula, rare.
7. „ tenuis, rare.
8. Syndosmia alba, rare.
9. Scrobicularia piperata, not numerous.

Fam. X.—Donacidæ.

10. Donax anâtinus, very common.

Fam. XI.—Mactridæ.

11. Mactra solida, common.
12. „ stultorum, semi-common.
13. „ subtruncata, rare.

Fam. XII.—Veneridæ.

14. Tapes decussata, common.
15. „ palustra, uncommon.
16. Cytherea Chione, ½ valve.
17. Venus striatula, very common.
18. „ fasciata, uncommon.
19. Artemis exoleta, semi-common.

Fam. XIII.—Cyprinidæ.

20. Cyprina Islandica, rare.

FAM. XIV.—CARDIADÆ.

21. Cardium rusticum, common.
22. " edule, common.
23. " Norvegicum, rare.

FAM. XV.—LUCINIDÆ.

24. Lucina flexuosa, ½ specimen, very rare.
25. " Leucoma, semi-rare.

FAM. XIX.—MYTILIDÆ.

26. Mytilus edulis, semi-common.
27. " barbata, rare.
28. " tulipa, rare.

FAM. XX.—ARCADÆ.

29. Nucula nucleus, rare.

FAM. XXII.—OSTREADÆ.

30. Pecten varius, common.
31. " maximus, uncommon.
32. " Danicus (1 specimen), very rare.
33. Anomia ephippium, semi-common.
34. Ostreas edulis, common.

Beds of oysters upon rocky and shingly bottoms are found to extend about eight miles, running from Cymmeran Bay to beyond Rhoscolyn to the north; great numbers are sent to the Manchester market.

CLASS IV.—PROSOBRANCHIATA.

FAM. XXVII.—CHITONIDÆ.

35. Chiton cinereus, rare.

FAM. XXVIII.—PATELLIDÆ.

36. Patella vulgata (and others), common.
37. " pellucida.
38. Acmea virginea, rare.
39. " testudinalis, common.

FAM. XXIX.—DENTALIADÆ.

40. Dentalium dentalis (Holyhead), rare.

FAM. XXX.—CALYPTRÆIDÆ.

41. Pileopsis Hungaricus, rare.

FAM. XXXI.—FISSURELLIDÆ.

42. Fissurella reticulata, rare.
43. Emarginula reticulata.

FAM. XXXIII.—TROCHIDÆ.

44. Trochus ziziphinus, very rare.
45. „ striatus.
46. „ cinerarius.
47. „ umbilicatus.
48. „ magus, rare.
49. Phasianella pullus, common.

FAM. XXXVII.—LILLORINIDÆ.

50. Littorina littorea, common.
51. „ rudis, „
52. „ littoralis, „
53. Rissoa crenulata, 1 specimen.
54. „ costata, few.
55. „ striata „
56. „ parva, numerous.
57. „ labiosa, few.
58. „ cingillus, few.
59. „ ulvae, 1 specimen

FAM. XXXVIII.—TURRITELLIDÆ.

60. Turritella communis, rare.

FAM. XXXIX.

61. Aporrhais pes-pelicani, Holyhead, 1 specimen.
62. Ceritbium reticulatum, rare.

FAM. XLI.—PYRAMIDELLIDÆ.

63. Chemnitzia elegantissima, rare.
„ fenestrata, rare.
64. Odostomia plicata, rare.

FAM. XLII.

65. Natica monilifera, very rare.

FAM. XLV.—MURICIDÆ.

66. Murex erinaceus, not very common.
67. Purpura lapillus, common.
68. Nassa reticulata, not very common.
69. Buccinum undatum, not common.

70. Fusus Islandicus (broken), rare.
71. Trophon clathratus, rare.

FAM. XLVI.—CONIDÆ.

72. Mangelia turricula, very rare.
73. „ rufa, rare.
74. „ nebula, rare.
75. „ costata, rare.
76. „ septangularis, rare.

FAM. XLVII.—CYPRÆADÆ.

77. Cypræa Europæa, common.

FAM. XLVIII.

78. Cylichna cylindracea (Holyhead 1), rare.
79. „ obtusa, rare.

Sponges—very few, common species.

Foraminefera—Polymorphina, Biloculina Lagena, Dentalina.

Zoophytes—few ordinary species.

Medusæ—seen in the luminous waves in the autumn—jelly fish and Portuguese man-of-war.

Hydrozoa—The most interesting of these beautiful creatures found in the bay are the crimson winged jelly fish, "*aurelia aurita,*" chiefly in the summer season, and the large species, *Rhizostoma Cuvieri,* in the spring. Many of the specimens I examined this Easter had a diameter of 14 inches, and weigh about 10lbs. They present a most exquisite appearance when alive, and are fine examples for dissection and study.

The Medusæ are in myriads in late summer, and give the bay a luminous and flashing star-like aspect on warm nights. The rarest and most striking of the Hydrozoa is *Physalia pelagica,* one of the Portuguese men-of-war. After a south-western gale, it is frequently met with by the oyster dredgers floating into the bay, and some of them are ultimately driven on shore amongst the rocks. I have found

large specimens, the bladder being 8 inches long, and the inferior appendages 24 inches. I know of no creature of the sea which is so exquisitely tinted with changing prismatic colours as this floating glassy bubble presents, and in my enthusiasm to examine into its structure, I had to suffer from the acute stinging secretions which in handling a Physalia is abundantly given out by the soft thread-like appendages; my hands and arms became inflamed, and it was several hours before the pain was allayed, leaving itching for days after.

I was not able to preserve any specimens in a recognisable form; they dry up to a mere skin and gelatinous film.

Starfishes—scarce, common Crossfish (uraster rubens), parts of Echinus, common.

Anelida—serpula (phyllodoce luminosa, lob-worm).

Crustacea—Lobster, abundant, edible Crab, common, very few others; Shrimp scarce.

Cirripedia—Acorn barnacle (Balani), fringing all the rocks to the line of high tide, and Lepas anatifera, occasionally.

The Anemones are abundant on the rocks and small pools, but the species are all common, such as *mesembryanthemum*.

Annual Meeting, May 3rd, 1875.

H. A. HURST, Esq., in the Chair.

The following gentlemen were elected Officers of the Section for the Session 1875-76 :—

President.

W. BOYD DAWKINS, F.R.S., &c.

Vice-Presidents.

R. D. DARBISHIRE, B.A., &c.
CHARLES BAILEY.
THOMAS ALCOCK, M.D.

Treasurer.

HENRY ALEXANDER HURST.

Secretary.

J. COSMO MELVILL, M.A., F.L.S.

Council.

JOSEPH BAXENDELL, F.R.A.S., &c.
SPENCER H. BICKHAM, JUN.
JOHN BARROW.
JOSEPH SIDEBOTHAM, F.R.A.S.
A. W. WATERS, F.G.S.
ALFRED BROTHERS, F.R.A.S.
THOMAS COWARD.

The following is the list of Members and Associates :—

Members.

Alcock, Thomas, M.D.
Bailey, Charles.
Barrow, John.
Baxendell, Joseph, F.R.A.S.
Bickham, Spencer H., Jun.
Binney, E. W., F.R.S., F.G.S.
Brockbank, W., F.G.S.
Brogden, Henry.
Brothers, Alfred, F.R.A.S.
Cottam, Samuel.
Coward, Edward.
Coward, Thomas.
Dale, John, F.C.S.
Dancer, John Benj., F.R.A.S.
Darbishire, R. D., B.A.
Dawkins, W. Boyd, F.R.S.
Deane, W. K.
Gladstone, Murray, F.R.A.S.
Heys, William Henry.
Higgin, James, F.C.S.
Hurst, Henry Alexander.
Latham, Arthur George.
Maclure, J. W., F.R.G.S.
Melvill, J. Cosmo, M.A., F.L.S.
Morgan, J. E., M.D.
Morris, Walter.
Nevill, Thomas Henry.
Piers, Sir Eustace.
Rideout, W. J.
Roberts, William, M.D.
Sidebotham, Joseph, F.R.A.S.
Smart, Robert Bath, M.R.C.S.
Smith, Robert Angus, Ph.D., F.R.S., F.C.S.
Vernon, George Venables, F.R.A.S.
Williamson, Wm. Crawford, F.R.S., Prof. Nat. Hist., Owens College.
Wright, William Cort.
Waters, A. W., F.G.S.

Associates.

Hardy, John.
Hunt, John.
Labrey, B. B.
Linton, James.
Meyer, Adolph.
Peace, Thos. S.
Percival, James.
Plant, John, F.G.S.
Rogers, Thomas.
Ruspini, F. Orde.
Stirrup, Mark.
Tatham, John F. W.
Warren, Hon. J. B. Leicester.
Waterhouse, J. Crewdson

THE MICROSCOPICAL AND NATURAL HISTORY SECTION OF THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY IN ACCOUNT WITH THE TREASURER, H. A. HURST.

Dr.

1875.	£	s.	d.
To Zoologist to 1861	5	0	0
„ Chairs and Rotating Table	6	2	0
„ Microscopical Journal	3	15	0
„ Subscription to Parent Society	2	2	0
„ Sundry Books—J. Friedlander	34	4	0
„ „ J. Rowsell	6	7	6
„ Printing	2	14	6
„ Subscription to Microscopical Journal	0	16	0
„ Teas and Postages	4	2	4
„ Balance at Bank	104	19	8
	£170	3	0

Cr.

1874.				£	s.	d.
May 23. By Balance at Bank				43	19	9
1875.—Subscriptions :						
Four Arrears	£2	0	0			
Ten Associates	5	0	0			
Thirty-six Members	18	0	0			
				25	0	0
Received on account of interest on Grant from late Natural History Society				100	0	0
Interest allowed by Bank to Dec., 1874				1	3	3
				£170	3	0

Examined and found correct,

(Signed) ARTHUR WM. WATERS, THOS. COWARD, } AUDITORS.

H. A. HURST.

1875.

May. By Balance at Manchester and Salford Bank, St. Ann's-street £104 19 8

PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER, Eng.

VOL. XV.

SESSION 1875-76.

MANCHESTER:

PRINTED BY THOS. SOWLER AND CO., RED LION STREET, ST. ANN'S SQUARE.

LONDON, BALLIÈRE, 219, REGENT STREET.

1876.

1877. Jan. 24,

[illegible]

[illegible]

NOTE.

THE object which the Society have in view in publishing their Proceedings is to give an immediate and succinct account of the scientific and other business transacted at their meetings to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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INDEX.

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PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 5th, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Thomas Mackereth, F.R.A.S., F.M.S., was elected an Ordinary Member of the Society.

"Glue Battery," by Dr. J. P. JOULE, F.R.S., &c.

If sulphate of zinc or sulphate of copper be dissolved in solution of gelatine and then carefully dried, an elastic solid is produced holding the salt in combination, which softens on the application of heat. I have taken advantage of this circumstance to form a voltaic couple which illustrates Faraday's discovery of the necessity of liquefaction for electrolysis, and which also may not be without some practical advantages.

I paint pieces of zinc with glue impregnated with salt of zinc, and pieces of copper with glue charged with sulphate of copper, dry, and lay them together in series. The pile thus formed is inert when cold, but capable of giving a good current when heated, as will be seen from the following results obtained with a single couple in connexion with a delicate galvanometer:

Temp. Fahr.	Deflection.	Temp. Fahr.	Deflection.
64°	0°	150°	5° 50′
70°	0° 40′	160°	7° 20′
80°	1° 10′	170°	8° 10′
90°	1° 40′	180°	10° 20′
110°	2° 50′	190°	17° 40′
130°	3° 0′	200°	43° 0′
140°	4° 40′		

After the lapse of three years I find that the above couple has retained its powers almost unimpaired.

E. W. Binney, F.R.S., V.P., said that many years ago, when he first came to Manchester, the red sandstone found under the city was known by the name of the Upper New Red Sandstone, the lowest division of the Trias and the equivalent of the Bunter. The authorities of the Geological Survey have divided it into Lower Soft Red Sandstone, Pebble Beds, and Upper Soft Red Sandstone; but the Pebble Beds are the deposits chiefly found under Manchester. Superior to this sandstone he had seen a deposit of red marls which he thought resembled the red marls of Cheshire, and at page 37 of the 1st vol. of the Transactions of the Manchester Geological Society he states that "this deposit, extending over so great a portion of the neighbouring county of Chester and reaching to a great thickness in the salt beds, is seldom to be seen in the south east of Lancashire. The only place where I have observed it is on the south of the town along the line of the Oxford Road from All Saints' Church to St. Peter's Square. After going through the diluvial clay a thin band of two feet in thickness occurs. In Chepstow Street leading out of Oxford Road this bed is between four and five feet in thickness. In the middle of it occurs a light coloured layer resembling fullers' earth in appearance, which briskly effervesces when treated with dilute hydrochloric acid. The lower part of the marl becomes arenaceous and passes

gradually into the upper sandstone. Both rocks dip to the W.S.W. at an angle of 8°. The marls here found are of a dark red colour, variegated by streaks and patches of green and yellow. They effervesce feebly with acids. No clearly recognized organic remains have been found in them—a circular concretion something resembling the cast of an ammonite was found in Chepstow Street." Since this notice was written, about 35 years ago, nothing has appeared respecting these marls. During the past summer he had noticed some shafts being sunk on the site of the intended new railway station of the Cheshire Lines in Back Newberry and Redford streets. In the first named locality the following section was met with:—

Till or Brick Clay	15ft.
Red and Variegated Marl	27
	42

In the last named

Till	15ft.
Red Marl...........................	41
	56

The red marls in their lower portions gradually became arenaceous and finally passed into the underlying red sand stone. In the pebble beds strata of red marls of one to two feet in thickness are sometimes met with, but nothing like a deposit of 40 feet. As the line of the Great Irwell fault of above 3,000 feet in throw runs a little to the south of the district where the marls are met with it is probable that these are a lower portion of the Red Marls of Cheshire lying above the Bunter beds of the Trias. He brought the matter before the Society for the purpose of directing the attention of the members to any excavations they might observe, as it was desirable that no facts concerning the rocks under our city should escape notice, and as so few opportunities occur for observing this deposit of marls no opportunity for examining them should be lost.

Ordinary Meeting, October 19th, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"On some Reactions of Bromine and Iodine," by Professor C. SCHORLEMMER, F.R.S.

Although it is well known that iodine dissolves in chloroform and some other liquids, such as carbon-sulphide and petroleum-naphtha, with a fine purple and bromine with a yellow colour, it seems not to be known that, under certain conditions, a colourless solution may be obtained containing both these elements in the free state.

Thus on adding dilute chlorine water drop by drop to a weak solution of potassium iodide and bromide, containing an excess of the latter, and shaking the liquid at the same time with chloroform, the purple colour of the iodine makes first its appearance, but gradually becomes fainter and at last disappears completely. It is, however, not easy to hit exactly the point, when the chloroform becomes quite colourless, but this is readily effected by adding so much chlorine water that the chloroform assumes a faint yellow tint, and then shaking the liquid with a cold and dilute solution of sodium bicarbonate, which must be carefully added drop by drop. It is easily proved that the colourless solution thus formed contains both free bromine and iodine, for on adding more bicarbonate, in order to remove the free bromine, the purple colour gradually appears again.

I am not able as yet to explain these facts; it is not a case of two complementary colours neutralising each other as I first assumed; for on mixing dilute solutions of the two elements in dry chloroform no colourless solution can be obtained. It seems that the presence of water has something

to do with it, for on adding the solution of bromine to that of chloroform until the purple is changed into a light brown, then shaking well with water, the colour of the chloroform becomes much paler and even disappears almost completely.

E. W. BINNEY, V.P., F.R.S, said that at the last meeting he had stated the circumstance of an urn containing bronze coins having been discovered under a peat moss. He now exhibited two small coins of bronze, about half an inch in diameter, evidently Roman from the figures on them, and most probably of the reign of Otho. They were found sixty years since under a peat bog of fifteen feet in depth, in an earthenware urn buried in the upper red marl of Misterton Car, in Notts, which forms the southern portion of the Level of Hatfield Chace, a large turf moss on the borders of the counties of York, Lincoln, and Nottingham. Some hundred coins were in the urn, and the two in his possession were presented to him about fifty years since by his relative the late Mrs. Joseph Hickson of East Stockwith, on whose property they were met with, and who furnished him with the above particulars. In the peat under which they occurred numbers of large oak and yew trees, with their roots attached to them, were as thickly placed as they could have grown on dry and strong soil. At the period when the urn was deposited, the spot was a dense forest, which was probably destroyed by the drainage of the district having been impeded by the raising of the beds of the rivers Trent, Ouse, and Idle, and thus causing the formation of the bog. The Dutch, under the direction of Sir Cornelius Vermuyden, and more modern drainers, have succeeded in bringing the land into a state fit for corn growing, but not capable of producing such oaks and yews as are now found, with their roots attached to them, lying in the peat.

"Notes bearing on Mathematical History," by Sir JAMES COCKLE, F.R.S., Corresponding Member of the Society.

1. To the list of Professor Boole's writings which follows the preface to the Supplementary Volume of his "Differential Equations" [2nd (posthumous) ed., 1865, pp. viii—xi] may be added a paper entitled "Mr. Boole's theory of the mathematical basis of logic," published in the *Mechanics' Magazine* (1848, vol. xlix, pp. 254-255).

2. Mr. Blissard in his "theory of generic equations" (*Q. J. of Math.* vol. iv, p. 279; vol. v, pp. 58 and 185, see also pp. 184 and 325) has applied "representative notation" very extensively. Professor J. R. Young had noticed (*Mech. Mag.*, 1847, vol. xlvii, p. 627) that the law of Bernoulli's numbers is expressed by

$$(1+B)^n - B_n = 0$$

if we write the exponents of B below instead of above that symbol. Judging from what purports to be an examination paper, I see that Prof. Young, examining for the Andrews Scholarship, 1851, at University College, London, again noticed (in question 7) the law.

3. In *Notes and Queries* (1854, vol. x, p. 48; see also p. 191; and 2nd Ser. vol. viii, p. 465, vol. ix, p. 340, vol. x, p. 162; and 4th Ser. vol. ii, p. 316) I long ago pointed out that Barocius, in the margin of p. 264 of his *Proclus* (1560) cites the *Geometricæ enarrationes* of Geminus, and I suggested that there might yet be some hope of recovering that work. In a comment (*ib.* 2nd Ser. vol. ix, p. 449) on something I had said, De Morgan remarked that he took "enarrationum" as a printer's mistake for "effectionum," if Heilbronner were right; adding that if there be, as both Petavius and Heilbronner seem to state, a printed catalogue of the manuscripts of Barocius, it would be desirable to revive the knowledge of it. De Morgan states that Petavius is the authority for manuscripts of Barocius being brought to England, and that it may be that a manuscript book of

Geminus, which Petavius describes as "nondum editus," yet exists in some English library. One of two strange things must have happened (see De Morgan, *ib.* p. 450), viz. either the minute Petavius omitted the title of the work, if it were given; or Heilbronner preserved a title from some other source, if it were not. I am not aware that the manuscript has been discovered.

4. Lagrange, at page 2 of vol. i of his "Mécanique Analitique" (1811), observes that the laws of statics are founded on general principles which may be reduced to three; that of the lever, that of the composition of forces, and that of virtual velocities. He says (*ib.* p. 18, No. 14) that the principle of the lever is the only one which has the advantage of being founded on the nature of equilibrium considered in itself, and as a state independent of motion.

5. Lagrange (*ib.* p. 20) says that it does not appear that the ancients knew of the law of virtual velocities. From what is said by Whewell in his "History of the Inductive Sciences" (1837, vol. ii, pp. 39 and 42; vol. i, pp. 69, 81, 82 and 94) it seems that the law, so far as it relates to the lever, was known to Aristotle. Whewell considered the physical philosophy of Aristotle (*ib.* i, 25) and of the Greeks (*ib.* 33) as an utter failure. He apparently regarded the doctrine of the lever (see vol. ii, p. 59; vol. i, chap. I, sect. 1, p. 91) with greater favour than that of virtual velocities (see vol. ii, chap. II, sect. 4, p. 39), in connection with which he makes no special mention of Lagrange (see vol. ii, p. 120). Professor Cayley (*Messenger*, N. S., vol. iii, p. 1) has given a dissertation "On the general equation of virtual velocities."

6, There are two modes of approaching the parallelogram of forces. First, we may from the composition of motions. through the second law of motion, pass to the composition of forces. Thomson and Tait, fortified by the example of Newton (see their 'Treatise,' &c., vol. i, pp. 181, 182, § 255—257) have followed this mode, which they believe

to contain the most philosophical foundation for statics. Lagrange, whose writings are a mine of historical information, says ('Méc. Anal.' i, 13) that the ancients knew the composition of motions, as we see by some passages of Aristotle, in his Mechanical Questions; and that the geometers especially have employed it for the description of curves, as, Archimedes for the spiral, Nicomedes for the conchoid, &c.

7. Secondly, we may seek to arrive at the composition of pressures, independently of the second law of motion, by processes which are valid whether that law be a law of nature or not, and which would be valid even if we had not any conception of motion, and which indeed do not render it necessary to consider whether pressure does or does not tend to produce motion. Lagrange ('Méc. An.' i, 19) thinks that the principle of the composition of forces, in being separated from that of the composition of motions, loses its principal advantages; and he, just before saying this, throws out a doubt as to whether a principle used by Daniel Bernoulli in his demonstration was altogether independent of the conception of motion. The whole subject of composition is discussed by De Morgan in his paper "On the General Principles of which the Composition or Aggregation of Forces is a consequence." (Camb. Trans. Vol X. Part II, 1859).

8. Lagrange ('Méc. An.' i, p. 14 No. 11) observes that, although the principles of the lever and of composition lead always to the same results, it is remarkable that the simplest case for the one becomes the most complicated for the other. He adds (*ib.* No. 12) that we can establish an immediate connection between these two principles by a theorem of Varignon. Newton's view is noticed by Lagrange (*ib.* No. 10).

9. Thomson and Tait have a special object ("Treatise" &c. vol. i, Preface p. v; p. 141 par. (f); p. 341 § 453), in reference to which Newton's proof may be the more appropriate or elegant. I here use the word "elegant" in the sense which

Austin, in his "Lectures on Jurisprudence" (vol. ii, p. 365) says it is used by the Roman lawyers. But there is much that is interesting in the other investigations on the subject.

10. Lagrange (*ib.* p. 29) uses the term "moment" in the sense which Galileo (*ib.* p. 20) gave it, viz: the product of the force into its virtual velocity; noticing however another (*ib.* p. 7) meaning viz. the product of the force into the arm of the lever by which it acts.

11. Whewell (Hist. Ind. Sci., i, 93), speaking of a "matter of obvious and universal experience," says

"This general fact is obvious, when we possess in our minds the ideas which are requisite to apprehend it clearly. When we are so prepared, the truth appears to be manifest, independent of experience, and is seen to be a rule to which experience must conform."

He seems (*ib.* ii, 25) to reiterate this opinion, which, if "appears to be manifest" be taken to mean "is self-evident," will hardly pass unquestioned.

12. Lagrange ('Méc. An.' i, 4, 5) tells us, in words which I translate as follows, to

"imagine a triangular plane loaded with two equal weights at the two ends of its base, and with a double weight at its vertex. This plane will evidently be in equilibrium, when supported by a straight line or fixed axis, which passes through the middle of the two sides of the triangle; for we may regard each of these sides as a lever loaded at its two ends with two equal weights, and which has its fulcrum on the axis which passes through its middle. Now we may contemplate this equilibrium in another manner, by regarding the base itself of the triangle as a lever of which the ends are loaded with two equal weights; and by imagining that there is a transverse lever which joins the vertex of the triangle and the middle of its base in the form of a T, and of which one end is loaded with the double weight placed at the vertex, and the other serves as the fulcrum of the lever which forms the base. It is evident that this last lever will be in equilibrium on the transverse lever which sustains it at its middle, and that the transverse lever will consequently be in equilibrium on the axis

on which the plane is already in equilibrium. But since the axis passes through the middle of the two sides of the triangle, it will pass also necessarily through the middle of the straight line drawn from the vertex of the triangle to the middle of its base; hence the transverse lever will have its fulcrum at the middle point, and must consequently be equally loaded at its two ends. Hence the load supported by the fulcrum of the lever which forms the base of the triangle, and which is loaded at its two ends with equal weights, will be equal to the double weight at the vertex, and consequently equal to the sum of the two weights."

13. Whewell in his "Mechanical Euclid" (2nd ed. p. 170) says that it will be found that Lagrange's proof, if distinctly stated, involves some such axiom as this:—that

"If two forces, acting at the extremities of a straight line, and a single force, acting at an intermediate point of the straight line, produce the same effect to turn a body about another line, the two forces produce at the intermediate point an effect equal to the single force."

He adds that though this axiom may be self-evident, it will hardly be considered as more simple than the proposition to be proved. Without discussing Whewell's criticism I observe that Lagrange ('Méc. An.' i, 16, 19) regards forces as quantities which can be added and subtracted, and which may be regarded (*ib.* p. 18) as weights. De Morgan (loc. cit.) seems to be of opinion that the proposition that the weight of the whole is equal to the sum of the weights of all the parts is known only by experience.

"Oakwal," near Brisbane,
Queensland, Australia,
July 22, 1875.

"Note on the Temperature of the Body during Physical Exertion," by M. M. Pattison Muir, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

In *Nature*, vol. xii, p. 132, appeared an account of Dr. Forel's observations on body-temperature during mountain

climbing, the general conclusion of which was that the temperature of the body *increases during* ascents or descents.

In the same journal, p. 165 of the same volume, Dr. Thorpe has published the results of observations made upon himself during an ascent of Mount Etna, from which it appears that he noticed a *small decrease* of temperature.

Dr. Anderson, however, *Nature*, vol. xii, p. 186, partially confirms the earlier experiments of Drs. Marcet and Lortet, who observed a *decrease of 3 or 4 degrees F.* in the temperature of the body during mountain ascents. The greatest fall recorded by Dr. Anderson amounts to 1°·6 F.

During the long vacation I carried out a few experiments upon my own body-temperature while rowing and while ascending a height.

The observations were made with a registering clinical thermometer, the bulb of which was placed in the mouth underneath the tongue. The bulb was allowed to remain in this situation during five minutes before the readings were noted.

Experiment I.

Initial temperature	98°·5 F.
After rowing for ½ hour	99°·05
After hard rowing for ¾ hours	98°·6
After resting ½ hour, eating two biscuits, and gently rowing for ¾ hours	99°

Experiment II.

Initial temperature	98°·4 F.
After ½ hour's hard rowing	99°
After 1 hour's ,, ,,	98°·7

Experiment III.—Ascent of Goatfell.

	Time.	Height in Feet.	Temp.
Beginning ascent	1 p.m.	——	98°·8
Easy climbing. Warm	1·50	900	99°
Stiffer climb. Perspiring	2·30	1,750	99°·5
Hard climbing. Perspiring much	2·45	2,200	99°·2
Very hard climbing. ,, ,,	3·0	2,750	99°
After descent of 2,500ft. Warm	4·0	——	99°·3

The observations were invariably taken *while actually rowing or climbing.*

These numbers are, in the main in accordance with those of Dr. Forel, and shew that a *slight increase* took place in my body-temperature during physical exertion. This increase was decidedly marked during the first half hour while rowing, and the first one and a-half hours while climbing, after which times a diminution of temperature took place, the final temperature being in no case, however, so low as the initial temperature.

Dr. Anderson, (loc. cit.) has attempted to explain the varying results of different observers by supposing that in some men the heat transformed into motion, and spent in doing work is restored by quick oxidation of material within the organism, while in the case of others whose power of oxidation is slower, no such equilibrium is maintained, and a decrease in body temperature consequently ensues. Looked at in this light the above numbers seem to show that during the earlier phases of physical exertion oxidation was more than sufficient to restore heat spent in doing work, but that as the work was continued and grew harder the excess of heat was gradually encroached upon. In the paper already cited from *Nature* (vol xii, p. 132) the abstractor shews that if body-temperature be taken as the exponent of internal work, then a greater increase ought to be observed during extra muscular work if unaccompanied by external work than if so accompanied. The results which I have obtained during canoeing bear out this theory. The following were the numbers :—

Experiment IV.

Initial temperature........................	98°·3 F.
After 20 minutes hard paddling.........	99°·4
After 30 „ „ „	99°·4

The external work done in propelling a light canoe through the water must be less than that needed for the

propulsion of a tolerably sized rowing boat—yet there is a very considerable amount of muscular work in maintaining the paddle in position, in the movements of the body, &c. Now the increase of temperature amounted to 1°·1 F., a much larger quantity than any noticed either during rowing or mountain climbing, where the *external* work is larger. Nor was the decrease which followed the first increase in the latter cases, noticeable while canoeing, probably because the external work was not continued long enough to make any great demand upon the internal heat.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 11th, 1875.

JOSEPH BAXENDELL, F.R.A.S., in the Chair.

Mr. PLANT exhibited specimens of Locusta migratoria, found in Ordsall Lane and Peel Park, Salford, after the unusually strong equinoctial gales of the end of September.

"On the Hybrid British Heath, *Erica Watsoni*, Benth.," by CHARLES BAILEY, Esq.

Some weeks ago Mr. E. W. Nix, M.A., when in Cornwall with his brother Mr. Arthur Nix, collected, on a wet, boggy moor in the neighbourhood of Truro, a heath, whose habit and characters struck him as being different both from *Erica Tetralix* and *E. ciliaris*, and both gentlemen were satisfied it was a hybrid form. A small specimen which Mr. Edward Nix brought home with him to Manchester was shown to me for identification, and I had no doubt in naming it as the *Erica tetralici-ciliaris* of Syme (=*E. Watsoni* of Bentham).

Nine years ago I spent a whole day in a careful but unsuccessful search for this plant in the same neighbourhood, and it was with much pleasure I saw Mr. Nix's specimen this year, as the only examples of this hybrid which I had previously seen were those which Mr. H. C. Watson has from time to time distributed through the Botanical Exchange Club. Mr. Watson's plant came originally from Cornwall, where it was collected by the late Rev. C. A. Johns many years ago, and this plant having been transplanted to Mr. Watson's garden, has been the principal source from which British botanists have obtained specimens. Knowing the rarity of the occurrence of this plant in a wild state, I urged Mr. Nix to procure a root from the locality where he discovered it, and to the united kindness of this gentleman and his brother the Section is indebted for a sight of the living plant now exhibited.

From a paragraph in the report of the Botanical Exchange Club, which has appeared during the last three weeks, we learn that Mr. Cunnack of Helston, with Mr. Blow, also found this hybrid last year, on a barren moor between Truro and Penryn.

Mr. Nix's plant has affinities with both its supposed parents. It differs from *Erica Tetralix* in its longer and larger corolla, which is pear-shaped rather than urceolate in form through a rather sudden contraction in the uppermost half; in its having a herbaceous bract at the base of each pedicel; and in its more numerous barren branches.

It differs from *Erica ciliaris* in its less leafy habit and more spreading branches; in its more raceme-like inflorescence; and in its stamens, which have two minute, shortly-ciliate, subulate awns. Its leaves have more affinity with those of *E. ciliaris* both in shape and pubescence, but it is destitute of all glands at the extremities of the cilia of the margin of the leaf; this feature is the more noteworthy as the cilia in Mr. Watson's specimens are nearly all

glandular. The styles are very slightly exserted and the uppermost portion does not gradually widen upwards as in *E. ciliaris,* but it is abruptly capitate as in *E. Tetralix,* and the ovary is downy, not glabrous, but I cannot find on the whole plant a single ovary which has come to maturity.

The character by which the hybrid appears to be differentiated from its allies the most readily is taken from the anther, and more particularly by its awn-like appendages. The anthers of *E. Tetralix* possess two awns which are equal in length to the lobes of the anther, while the filament is united to the anther at the back just above the base of the lobes. The anthers of *E. ciliaris* are also attached by their bases to the filament, but there is no trace of any awns. In the hybrid plant however, the attachment of the filament is higher up the back of the papillose anther, so that the two lobes project much more than in the parent plants; from each side of the filament at its junction with the anther, there rises a slender awn whose length is only one-fourth that of the anther, and these two awns do not project beyond the lobes as they do in *Erica Tetralix.*

From a comparison of the specimens exhibited, it will be seen that Mr. Nix's plant comes nearer to *E. ciliaris* than do Mr. Watson's, particularly in the infloresence; but they agree perfectly in the character of the awns of the anther, as noted above. In habit it seems to have some affinity with *E. Mackaiana,* and it is nearer *E. Tetralix* than *E. Ciliaris* in this respect.

The habitat of the hybrid plant is described by Mr. Nix as decidedly boggy, and his observation of the Cornish localities of *E. ciliaris* leads him to conclude that it also flourishes best in a similarly moist station. Dr. Syme on the other hand gives "sandy heaths" as the character of the station of *E. ciliaris. Erica Tetralix* loves boggy land, and this seems to be the character of the station where *Erica tetralici-ciliaris* flourishes in some abundance.

The Connemara station for *Erica Mackaiana* is peculiar; according to my observation it occurs only on a small rocky eminence which rises out of a vast bog which is the home of *E. Tetralix*; as it grows in free flowering bushes a foot or more in height, it may readily be picked out from plants of *E. Tetralix*.

A series of British and Continental specimens of the heaths named in this communication now lie on the tables for comparison with the living plant.

Ordinary Meeting, November 2nd, 1875.

R. Angus Smith, Ph.D., F.R.S., &c., Vice-President, in the Chair.

Peter Spence, F.C.S., &c., exhibited a piece of 2 to 3 inch lead pipe in which the metal had been entirely transformed into galena, the crystalisation being visible through the whole of the specimen.

The shape of the lead pipe was unaltered, showing that the lead had not been exposed to a melting heat, no increase of bulk was visible, but the pipe was so brittle as to shiver with a blow. The circumstances in which this change was effected, as nearly as can be made out, were as follows.

The pipe had been used for the conveyance of gas ammoniacal water and was sunk under ground. It was in the vicinity of a furnace which heated the ground where it lay. It had been disused for some years but never taken up. When the ground where it lay had to be excavated for a new erection, it was found that there had been a considerable leak of gas water, as the ground for some space was impregnated with ammoniacal salts. About 25 per cent of the ammonia in gas water being sulphide, and the ground being warm, a constant atmosphere of sulphide of ammonium would surround the pipe, and this seems to have been the cause of the conversion of the lead into sulphide, as only that part of the pipe which was in the vicinity of the leak was found to be transformed.

"On the Principle of the Electro-Magnet constructed by Mr. John Faulkner," by Professor Osborne Reynolds, M.A.

The magnet which forms the subject of this paper consists of a soft iron bar with a flat plate attached to one end and surrounded by a coil of wire in the same way as the ordinary electro-magnet. Outside this coil is placed a tube of soft iron of the same length as that portion of the interior bar which projects beyond the plate; this tube has flat ends —one of which is in contact with the plate, while the other comes up flush with the end of the bar—so that a plate or keep placed over the end is in contact with both the bar and the cylinder. The magnet is excited in the ordinary way, by connecting the ends of the wire which forms the coil with the poles of a battery. When thus excited this magnet exhibits certain peculiarities as compared with a common magnet. In the first place the magnetic field is very limited, being confined to the space in front of the open end of the tube, there is little or no magnetism along the tube or at the closed end. The magnet retains its keep with greater force than the simple bar. Mr. Faulkner has some magnets of this kind which retain the keep with 100 times more force when the outer tube is on than when it is removed. The ratio of these retaining powers appears, however, to depend on the relative diameters of the bar and the tube; the larger the bar in proportion to the tube, the greater is the difference. Some magnets, made especially to test the relative powers, give an increase of only double as compared with the simple bar magnet. This magnet has a greater sustaining power than the horse-shoe magnet. This was shown by putting tubes round the poles of a horse-shoe magnet, by which means it was made to sustain greater weights than it would without the tubes.

The object of the paper was to suggest explanations of these phenomena. They were attributed to three principal causes.

1. The tube surrounding the bar unites the poles and converts the magnet into a kind of horse-shoe magnet, the

ends of the tube having the opposite polarity to those of the bar. If this were all, however, this magnet would not have any advantage over the horse-shoe magnet.

2. The close proximity of the tube to the bar enables the one pole to exert greater inductive action on the other, than in the case of the horse-shoe form.

3. The electro-magnetic action of both sides of the coil is utilised in the same manner as in the astatic galvanometer. The current converts the tube into a magnet of opposite polarity to the bar, and hence these two magnets act upon each other by induction, which their relative positions enables them to do with the greatest effect. It appears from the researches of Dr. Joule that the larger the bar inside the coil the less will be the intensity of magnetism exerted in it by the coil. While it may be shown that the smaller the tube—the closer it is to the coil—the greater will be the intensity of magnetism excited in it. Also the inductive action of the iron in the tube on that of the bar is inversely proportional to the square of the distance between them. Hence it follows that the effect of using the outside as well as the inside of the coil must increase rapidly as the diameter of the bar approximates to the internal diameter of the tube.

After the reading of the paper, Mr. FAULKNER exhibited some of his magnets; and by means of iron filings scattered on sheets of paper produced some very beautiful diagrams illustrating the effect of the outside tube on the magnetic field.

General Meeting, November 16th, 1875.

Rev. WILLIAM GASKELL, M.A., Vice-President, in the Chair.

Mr. John Boyd, of Victoria Park, and Mr. E. W. Nix, M.A., of the Branch Bank of England, were elected Ordinary Members of the Society.

Ordinary Meeting, November 16th, 1875.

Rev. William Gaskell, M.A., Vice-President, in the Chair.

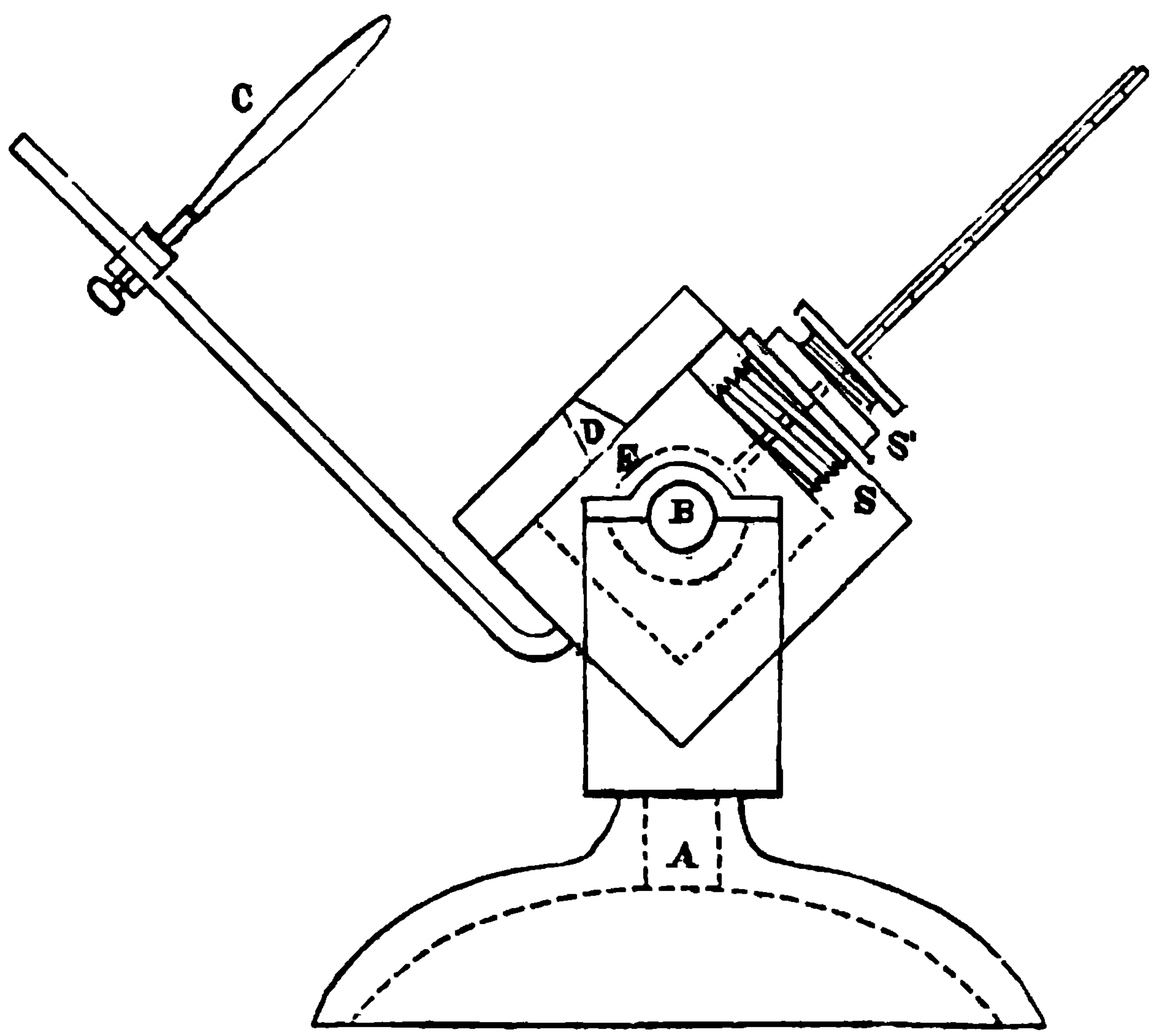
"On an Instrument for Measuring the Direct Heat of the Sun," by Professor Balfour Stewart, LL.D., F.R.S.

The instrument generally employed for giving the radiant energy of the sun's rays acts upon the following principle. *In the first place* the instrument is sheltered from the sun but exposed to the clear sky, say for five minutes. Let the heat so lost be termed r. *Secondly,* the instrument is turned to the sun for five minutes. Let the heat so gained be termed R. *Thirdly,* the instrument being now hotter than it was in the first operation is turned once more so as to be exposed to the clear sky for five minutes while it is shielded from the sun. Let the heat so lost be termed r'.

It thus appears that r denotes the heat lost by convection and radiation united when the instrument, before being heated by the sun, is exposed for five minutes to the clear sky, while r' denotes the heat lost by these same two operations by a similar exposure after the instrument has been heated by the sun; and it is assumed that the heat lost from these two causes during the time when the instrument is being heated by the sun will be a mean between r and r', and hence that the whole effect of the sun's rays will be in reality $R + \frac{r + r'}{2}$.

Now although this assumption may in the average of a great number of experiments represent the truth, yet in many individual cases it may be far from being true. It would therefore seem to be desirable to get rid of this uncertainty by constructing an instrument in which we are sure that the causes of variability are not allowed to operate.

These causes of variability I have attempted to get rid of in the following manner. With the help of Mr. Jordan, mechanician at Owens College, the following instrument has been constructed. It consists of a large mercurial thermometer with its bulb in the middle of a cubical cast iron chamber, this chamber being of such massive material that its temperature will remain sensibly constant for some time.



The chamber with its thermometer has a motion in azimuth round a vertical axis A, and also a motion in altitude round a horizontal axis B. A three inch lens C of 12 inches focal length is attached by means of a rod to the cubical chamber so as to move with it. The nature of this attachment will be seen in the figure. Thus the whole instrument may be easily moved into such a position that the lens as well as the upper side of the chamber which is parallel to the plane of the lens may face the sun, and an image of the sun be thrown through a hole D in the side of the chamber upon the thermometer bulb E.

The stem of the thermometer protrudes from the chamber as in the figure. A screw S somewhat larger in diameter than the bulb of the thermometer is made use of to attach the thermometer to its enclosure, and a smaller screw S′ pressing home upon india rubber washers enables the thermometer to be properly adjusted and kept tight when in adjustment.

In the present instrument the internal diameter of the chamber is 2 inches, while the bulb of the thermometer is about 1¼ inches in diameter.

The scale of the thermometer is very open, more than an inch going to one degree. I have generally allowed the image of the sun given by the lens to heat the thermometer bulb for one minute, during which time an increase of temperature, not exceeding in any case two degrees, has been produced.

As far as principle is concerned there appears to be no objection to the present instrument, nevertheless it is open to a very serious practical objection. The scale being so very open, the stem comprehends only a few degrees; frequently, therefore, the temperature is such that the extremity of the mercurial column is either below or above the stem. Now the thermometer has a small upper chamber, and by means of a method of manipulation well known to those who work with thermometers, it is possible to add to or take away from the main body of mercury in the bulb so as to keep the end of the mercurial column always in the stem. But experience has convinced me that for a thermometer with such a large bulb, frequent manipulation of this kind is not unattended with danger to the bulb. On this account the instrument in its present form is, I conceive, unsuited for steady work in an observatory from year to year.

It is however possible without any appreciable sacrifice of the scientific principle of the instrument to alter it in

such a manner as to remedy this defect. Without altering the size of the bulb, I should propose for a permanent instrument a stem say 18 inches long with a bore of such diameter that the stem should embrace a range of temperature between 20° Fahr. and 92° Fahr. Thus somewhat less than five degrees will go to the inch. The stem might be protected from the risk of accident by an appropriate shield. Let such a thermometer be heated for two minutes and the size of the lens be somewhat increased. In this case a rise of something like 5° Fahr. will be obtained, and this heating effect might very easily be estimated to one hundredth of the whole, while the same thermometeı would serve for all the temperatures likely to occur in these islands during the course of the year.

I ought to add that a pasteboard cover, gilded on the outside is made to surround the chamber, and also that between the lens and the chamber there is a pasteboard shield with a hole in it to permit the full rays from the lens to pass—the object of this shield being to prevent rays from the sun or sky from reaching the instrument.

In such an instrument r or the change taking place in the thermometer before exposure to the sun will in all probability completely disappear, while r' will be extremely small. At any rate we may be quite certain that $R + \frac{r + r'}{2}$ will accurately represent the heating effect of the sun.

We may probably suppose that in the same instrument the lens (which must always be kept clean) will always stop the same or nearly the same proportion of the solar rays. But the lens of one instrument may not stop the same proportion as that of another instrument. This, however, is no objection if it be borne in mind that the instrument is a differential one. In practice there would be some standard instrument which would be retained at a central observatory, and all other instruments would, before being issued, be

compared with it. It would be thus possible to compare together the indications of various instruments working in different places provided that these before being issued had their co-efficients determined at the central observatory.

"On a Colorimetric Method for determining small quantities of Copper," by THOMAS CARNELLEY, B.Sc., F.C.S., Demonstrator in the Chemical Laboratory of Owens College. Communicated by Professor H. E. ROSCOE, F.R.S., &c., &c.

Last year I brought before this Society a paper (Proceedings Vol. XIV., 2) on a colorimetric method for determining iron in waters, and as this method has been found convenient for estimating small quantities of iron in substances other than water,* I thought it would likewise be useful to have a delicate and easy method of a similar kind for copper, and it is the description of such a method that forms the subject of the present paper.

The reagent used is the same as in the case of iron, viz., potassium ferrocyanide, which gives a purple-brown colour with very dilute solutions of copper. This reaction, however, is not so delicate as it is with iron, for 1 part of the latter in 13,000,000 parts of water can be detected by means of potassium ferrocyanide, while 1 part of copper in a neutral solution, containing ammonium nitrate, can be easily detected in only 2,500,000 parts of water. Of the coloured reactions which copper gives with different reagents, those with sulphuretted hydrogen and potassium ferrocyanide are by far the most delicate, and as a preliminary the comparitive values of these two reagents were tested with the following results, the determination being made in each case in 150cc. of water:—

(1) *With* H_2S. 1 part of copper produces a colour in 2,500,000 parts of water.

* Among others I may mention that use has been made of this method by Wanklyn, in the indirect determination of Alum in Bread.—*Chemical News*, Vol. XXXI., p. 67.

(2) *With* K_4FeCy_6

(*a*) In acid solutions, the colour produced being earthy brown, 1 part of copper produces a colour in 1,000,000 parts of water.

(*b*) In neutral solutions, the colour being purple-brown, 1 part of copper produces a colour in 1,500,000 parts of water.

(*c*) In neutral solutions containing ammonium nitrate, the colour being purple-brown, 1 part of copper produces a colour in 2,500,000 parts of water.

From the above it will be seen that of the two reagents, sulphuretted hydrogen is the more delicate, except in the latter case when they are of equal value. But potassium ferrocyanide has a decided advantage over sulphuretted hydrogen in the fact that lead, when not present in too large quantity does not interfere with the depth of colour obtained, whereas to sulphuretted hydrogen it is, as is well known, very sensitive.

And though iron if present would, without special precaution being taken, prevent the determination of copper by means of potassium ferrocyanide, yet by the method as described below the amounts of these metals contained together in a solution can be estimated by this reagent.

As the above results show ammonium nitrate renders the reaction much more delicate; other salts, as ammonium chloride and potassium nitrate, have likewise the same effect.

The method of analysis consists in the comparison of the purple-brown colours produced by adding to a solution of potassium ferrocyanide—first, a solution of copper of known strength, and secondly, the solution in which the copper is to be determined.

The solutions and materials required are as follows :—

(1) *Standard copper solution.*—Prepared by dissolving 0·393 grm. of pure $CuSo_4{\cdot}5H_2O$ in one litre of water. 1cc. is then equivalent to 0·1 mgrm. Cu.

(2) *Solution of ammonium nitrate.*—Made by dissolving 100 grm. of the salt in one litre of water,

(3) *Potassium ferrocyanide solution.*—Containing 1 part of the salt in 25 parts of water.

(4) *Two glass cylinders* holding rather more than 150cc. each, the point equivalent to that volume being marked on the glass. They must, of course, both be of the same tint and as nearly colourless as possible.

(5) *A burette*, marked to $\frac{1}{10}$cc., for the copper solution; a 5cc. pipette for the ammonium nitrate, and a small tube to deliver the potassium ferrocyanide in drops.

The following is the method of analysis:—Five drops of the potassium ferrocyanide are placed in each cylinder, and then a measured quantity of the neutral solution in which the copper is to be determined into one of them (A), and both filled up to the mark with distilled water, 5cc. of the ammonium nitrate solution added to each and then the standard copper solution run gradually into (B), till the colours in both cylinders are of the same depth, the liquid being well stirred after each addition. The number of cubic centimetres used are then read off. Each cubic centimetre corresponds to 0·1 mgrm. of copper, from which the amount of copper in the solution in question can be calculated.

The solution in which the copper is to be estimated must be neutral, for if it contains free acid the latter lessens the depth of colour and changes it from a purple brown to an earthy brown. If it should be acid it is rendered slightly alkaline with ammonia, and the excess of the latter got rid of by boiling. The solution must not be alkaline, as the brown coloration is soluble in ammonia and decomposed by potash; if it is alkaline from ammonia this is remedied as before by boiling it off; while free potash, should it be present, is neutralized by an acid and the latter by ammonia.

Within moderate limits the amount of potassium ferrocyanide does not affect the accuracy of the method as was proved by several experiments, for instance, when ½cc. and 2cc. of the ferrocyanide were added to the two cylinders respectively, water up to the mark, and 5cc. of ammonium nitrate to each, then 7cc. of the standard copper solution produced in each an equal depth in colour.

The same may be said of the ammonium nitrate, for in one of several trials, all leading to the same result, when there were five drops of ferrocyanide in each cylinder, with water up to the mark, and 5cc. of ammonium nitrate in one and 15cc. in the other, an equal depth of colour was obtained on running into each 7cc. of the standard copper solution.

The following are the results obtained by estimating the copper in pure solutions of copper sulphate of known strength:—

Copper found.	Copper calculated.
205·00 mgrm.	202·08 mgrm.
32·36 "	31·32 "
6·50 "	6·27 "
1·18 "	1·13 "
1·13 "	1·21 "
0·96 "	1·01 "
0·75 "	0·75 "
0·59 "	0·61 "
0·52 "	0·50 "
0·42 "	0·40 "
0·39 "	0·38 "
0·22 "	0·20 "
0·13 "	0·13 "
0·12 "	0·10 "
0·055 "	0·050 "

In order to test the effect which the different salts might have on the accuracy of the method, 8·0 grms. of a mixture of the following salts, viz.:—Ammonium chloride, sodium chloride, potassium nitrate, calcium chloride, calcium sulphate,

and magnesium sulphate, were dissolved with an amount of copper sulphate, containing 0·101 grm. Cu. to 1 litre. Varying quantities of this solution were taken, and the copper estimated therein with the following results, from which it is seen that these salts have no detrimental effect. :—

Copper found.	Copper calculated.
0·54 mgrm.	0·51 mgrm.
0·71 „	0·71 „
0·91 „	0·91 „

In the same manner the effect of the presence of colourless non-volatile organic matter was tested by dissolving up 13 grms. of sugar with an amount of copper sulphate equivalent to 0·0505 grm. copper in 1 litre of water, and the copper estimated in two different portions as before, the following numbers being obtained :—

Copper found.	Copper calculated.
0·52 mgrm.	0·51 mgrm.
0·82 „	0·81 „

In order to see what influence the presence of lead might exercise on this method of estimating copper, a quantity of the sulphate containing 0·255 grm. Cu. was dissolved in water, the copper precipitated by potash, washed, and the oxide dissolved in nitric acid and the solution after nearly neutralizing with ammonia diluted to 1 litre with the addition of 2 grm. of lead nitrate = 1·25 grm. Pb. Varying quantities of this solution were taken, and the copper in them estimated with the following results :—

Copper found.	Copper calculated.
0·80 mgrm.	0·77 mgrm.
0·75 „	0·70 „
0·51 „	0·49 „
0·49 „	0·51 „
0·38 „	0·35 „

From which it will be seen that lead when present in not too large quantity has little or no effect on the accuracy of the method. The precipitate obtained on adding potassium

ferrocyanide to a lead salt is white, and this, except when present in comparatively large quantity with respect to the copper, does not interfere with the comparison of the colours. In the above experiments the proportion of lead to copper was as 5 to 1.

When copper is to be estimated in a solution containing iron the following is the method of procedure to be adopted. To the solution a few drops of nitric acid are added in order to oxidise the iron, the liquid evaporated to a small bulk and the iron precipitated by ammonia. Even when very small quantities of iron are present this can be done easily and completely if there is only a very small quantity of fluid. The precipitate of ferricoxide is then filtered off, washed once, dissolved in nitric acid and reprecipitated by ammonia, filtered, and washed. The iron precipitate is now free from copper, and in it the iron can be estimated by dissolving in nitric acid, making the solution nearly neutral with ammonia and determining the iron by the method given in the paper before referred to. The filtrate from the iron precipitate is boiled till all the ammonia is completely driven off, and the copper estimated in the solution so obtained as already described. The following are the results obtained with solutions containing known quantities of iron and copper: —

	Copper		Iron	
	Found	Calculated	Found	Calculated
(1)	0·53 mgrm.	0·51 mgrm.	0·22	0·20 mgrm.
(2)	0·69 „	0·61 „	2·15	2·40 „
(3)	0·79 „	0·76 „	2·42	3·00 „
(4)	0·66 „	0·66	——	——

When the solution containing copper is too dilute to give any coloration directly with potassium ferrocyanide, a measured quantity of it must be evaporated to a small bulk and filtered if necessary, and if it contains iron, also treated as already described.

In the determination of copper and iron in water, for which the method is specially applicable, a measured quantity is evaporated with a few drops of nitric acid to dryness, ignited to get rid of any organic matter that might colour the liquid, and dissolved in a little boiling water and a drop or two of nitric acid, if it is not all soluble it does not matter; ammonia is next added to precipitate the iron, the latter filtered off, washed, redissolved in nitric acid, and again precipitated by ammonia, filtered off and washed. The filtrate is added to the one previously obtained, and the iron estimated in the precipitate and the copper in the united filtrate.

The distilled water used in the Owens College Laboratory, and which is condensed by the apparatus made by Hirzel, of Leipzig, gave, on analysis by the above method, the following results, two litres of the water being used for the purpose:—

$$\left.\begin{array}{l}0{\cdot}15 \text{ parts Cu.}\\ 0{\cdot}03 \text{ parts Fe.}\end{array}\right\} \text{in 1,000,000 parts of water.}$$

The copper and iron in this case were evidently derived from the fittings of the condensing apparatus, which consisted in great part of these metals.

Ordinary Meeting, November 30th, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"On the Estimation of very small quantities of Lead and Copper," by M. M. PATTISON MUIR, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

As I have lately been occupied with experiments upon the action of saline solutions upon lead and copper, which involved the measurement of very small quantities of these metals I thought it might be well to test the accuracy and delicacy of the method employed.

The method itself is in no way new, being that described by Wanklyn in his Book on "Water Analysis." The depth of colour produced by the addition of sulphuretted hydrogen water to a known volume of the liquid under examination is compared with the colour produced, by the same means, in an equal volume of water to which a known amount of lead or copper, in solution, has been added. In comparing the colour of the liquid under examination with the standard liquid I find it preferable to employ stout glass tubes, holding about 100 cc. and having a diameter of about 1·5 cm., rather than white porcelain dishes as recommended by Wanklyn. The contents of the tubes are thoroughly mixed by means of glass tubes on the ends of which bulbs have been blown. (See *Thorpe on a method of estimating nitric acid, &c.* Chem. Soc. J. [2] XI. 547.)

Wanklyn recommends the use of standard solutions, 1 cc. of which is equal to 1 mgm. of copper or of lead: he employs 70 cc. of the water to be tested. If, therefore, the colour produced on adding sulphuretted hydrogen water to 70 cc. of the liquid under examination is found to be equal

to that produced by the addition of the same reagent to 70 cc. of distilled water to which 1 cc. of the standard has been added, we shall have 1 grain per gallon of lead in the water. But one-tenth of a grain of lead per gallon is generally considered hurtful when present in a drinking water; to estimate this we should require to use only 0·1 cc. of the standard: a very small error in reading the burette measurements would introduce a comparatively large error in the result. Thus in the case of a water containing one-tenth grain of lead per gallon an error in reading of 0·05 cc. would introduce an error in the quantity of lead equal to one-half of the total quantity to be estimated. The first point, therefore, to investigate appeared to be *the strength of the standard solutions.* I shall describe the experiments made with copper.

Standard used 1 cc. = 1 mgm. copper.

Expt.	Taken.	Found.
No. 1	5 mgm. per litre	4 mgm. per litre.
No. 2	2·5 ,, ,,	2 ,, ,,

Standard used 1 cc.=0·1 mgm. copper.

	Taken.	Found.
No. 3	5 mgm. per litre	4·8 mgm. per litre.
No. 4	2·5 ,, ,,	2·4 ,, ,,
No. 5	0·5 ,, ,,	0·5 ,, ,,

In each case 50 cc. of liquid was used.

Similar results were obtained with lead solutions.

The use of a standard, 1 *cc. of which is equal to* 0·1 *mgm.*, of copper or of lead enables more accurate and more delicate results to be obtained than the use of a stronger standard does.

The second point to be determined was *the limits of accuracy of the method,* and first as to the lower limit.

From the experiments with copper already detailed it will be seen that 0·5 mgm. of copper per litre could be estimated by using 50 cc. of the liquid.

Standard used 1 cc. = 0·1 mgm. copper.

Solution contained 0·25 mgm. of copper per litre. Details as before.

Added 0·3 cc. standard.		Colour too dark.
Added 0·2 cc.	"	Colour rather too dark.
Added 0·1 cc.	"	Colour equal to the other.

	Taken.	Found.
No. 6	........0·25 mgm. per litre.	......0·20 mgm. per litre.

In this experiment it was very difficult to determine the exact point at which the colours were the same, as the intensity of coloration produced was very slight. A further addition of 0·05cc. of the standard could hardly be said to produce a noticeable change in the depth of colour.

I think, therefore, that 0·5 mgm. of copper per litre = 0·035 grains per gallon is the smallest quantity which can be accurately estimated by this process when working with 50 cc. of the liquid under examination.

The amount of lead which can be estimated with accuracy is less minute than the amount of copper.

Standard used 1cc. = 0·1 mgm. lead.

	Taken.	Found.
No. 7	.........0·25 mgm. per litre	...no coloration.
No. 8	.........0·5 " "	0·4 mgm. per litre.
No. 9	.........0·75 " "	0·6 " "
No. 10	.........1·0 " "	1·0 " "

1 mgm. of lead per litre = 0·07 grains per gallon, is, therefore, the smallest quantity which can be accurately estimated by this process when working with 50cc. of the liquid under examination.

By the evaporation of 1 litre of water to 50cc., a quantity of copper so small as 0·025 mgm. per litre, or of lead equal to 0·05 mgm. per litre, can be estimated by this process. In other words, the process will estimate 1 part of copper in 2,000,000 parts of water, or 1 part of lead in 1,000,000 parts of water.

Secondly, as to the upper limit.

Standard used 1cc. = 0·1 mgm. copper.

	Taken.	Found.
No. 11.........	20 mgm. per litre...	20·4 mgm. per litre.
No. 12.........	10 „ „	10 „ „
No. 13.........	25 „ „	24—28 „ „
No. 14.........	30 „ „	28—32 „ „

20 mgm. of copper per litre = 1·4 grains per gallon is the largest quantity which can be estimated by this method when working with 50cc. of the liquid under examination.

With lead the following results were obtained :—

	Taken.	Found.
No. 15.........	10 mgm. per litre...	10 mgm. per litre.
No. 16.........	12 „ „	12—15 mgm. per litre.
No. 17.........	15 „ „	colour too dark to allow of estimation.

10 mgm. of lead = 0·7 grains per gallon, is, therefore, the largest quantity which can be estimated by this method when working with 50cc. of liquid.

In making these determinations, I found that the colours of the liquids might be compared *immediately* after the addition of sulphuretted hydrogen. The colours did not become intensified on standing.

I also found that it was immaterial whether the standard was added before the sulphuretted hydrogen water or *vice versa*. Thus there is no need if the colour of the standard be too light, to prepare a fresh standard, as must be done in nesslerising. It is only necessary to add another measured quantity to the liquid which already contains sulphuretted hydrogen.

The addition of one or two drops of dilute hydrochloric or nitric acid in no way affected the accuracy or delicacy of the estimation of copper. In the case of lead, a drop of hydrochloric acid caused a faint turbidity (especially in estimating large quanities of the metal), which interfered materially with the results. If an acid must be added, acetic acid is, I think, the best.

When working with 50cc. of liquid, so small a quantity as 0·5 mgm. of copper, or 1 mgm. of lead per litre, may be estimated by this process. If it is required to estimate smaller quantities than these, the liquid must be concentrated by evaporation. If the amount of copper exceed 20 mgm., or of lead exceed 10 mgm. per litre, a smaller quantity of the liquid than 50cc. must be used.

"On Certain Circumstances which affect the Purity of Water supplied for Domestic Purposes," by M. M. Pattison Muir, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

Water as supplied for domestic use may suffer contamination from various sources. Those which I propose to consider are (1) the metallic pipes through which the water flows, and the metallic vessels in which it may be stored, (2) certain of the metallic vessels through which the water may pass during various domestic processes, and (3) the existence of cisterns inside the house in which the water may be stored before it is used.

The metals which are most commonly employed in the formation of water pipes, or of vessels in which water is kept, are lead and copper: these metals exert, as is well known, a poisonous action upon the human organism.

It is known that water exerts a certain solvent action upon these metals, and that this action varies in accordance with the quality and quantity of the salts held in solution by the water. I have endeavoured to obtain a few definite measurements of this action in regard to (*a*) the nature of the salts in solution, (*b*) the quantity of those salts, and (*c*) the length of time during which the action proceeds.

I. *Action on Lead.*

A number of solutions were made containing a known amount of various salts dissolved in distilled water: pieces of clean bright lead were suspended in these liquids for

various lengths of time, and the amount of lead which was dissolved was estimated at certain intervals; the method employed being the colorimetric one described in the foregoing paper. The salts employed, the amounts of each, and the amount of lead dissolved after 24, 48, and 72 hours' action are stated in the following table in mgrms. per litre and in grains per gallon. The surface of lead exposed measured 5600 sq. mm.

TABLE A.

Lead dissolved by water containing various salts in solution.

Salt.	Mgms. per litre.	Grains per gall.	Lead dissolved. In Mgms. per Litre. After 24	48	72	In Grains per Gallon. 24	48	72 hrs.
Ammonium Nitrate	20	1·4	18·0	...	25·0	0·91	...	1·75
Ditto	40	2·8	15·0	15·0	32·0	1·05	1·05	2·24
Ditto	80	5·6	15·0	...	...	1·05	...	...
Potassium Nitrate and Sodium Sulphate ...	20 and 50	1·4 and 3·5	2·0	2·0	...	0·14	0·14	...
Potassium Nitrate and Sodium Sulphate ...	40 and 212	2·8 and 14·7	0·8	1·0	1·2	0·05	0·07	0·08
Potassium Nitrate and Potass. Carbonate...	45 and 305	3·1 and 21·5	...	...	0·3	...	...	0·021
Potassium Nitrate and Potassium Sulphate	70 and 504	5·4 and 35·2	...	...	0·5	...	...	0·035
Calcium Sulphate...	252	17·5	0·4	...	0·8	0·02	...	0·05
Ditto	408	28·5	0·4	...	1·0	0·02	...	0·07
Potass. Carbonate ..	310	21·7	...	...	0·2	...	...	0·014
Ditto	516	36·1	...	...	0·2	...	...	0·014
Calcium Chloride ...	250	17·5	0·5	0·5	0·5	0·04	0·04	0·04
Ditto	510	35·7	0·3	...	0·4	0·02	...	0.028
Sodium Sulphate ...	200	14·0	...	...	0·8	...	...	0·05
Ditto	400	28·0	...	...	0·5	...	...	0·03
Ammonium Nitrate and Calcium Chloride ...	20 and 60	1·4 and 4·2	...	...	1·8	...	...	0·126
Ammonium Nitrate Potass. Carbonate and Sodium Sulphate ...	20 100 and 200	1·4 7·0 and 14·0	...	...	0·4	...	...	0·028
Sodium Sulphate Potass. Carbonate and Calcium Chloride ...	200 40 and 100	14·0 2·8 and 7·0	...	...	0·1	...	...	0·007
Loch Katrine water			1·0	1·0	1·5	0·07	0·07	0·105
Distilled water			2·0	2·0	3·0	0·15	0·15	0·210

From this table it is evident that the salts enumerated

have, when in solution, very different actions upon lead. Nitrates undoubtedly very largely increase the solvent action of water upon lead: the other salts generally diminish this action.

The general conclusion which I would draw from these results are:—

(1.) *Nitrates* if present alone even in small quantity cause water to exert a very marked solvent action upon lead.

(2.) The presence of other salts—*sulphates, carbonates,* and *chlorides—along with nitrates,* greatly decreases, or even stops, this solvent action: carbonates especially exercise a deterrent action.

(3.) *Carbonates, sulphates,* and *chlorides,* when added to distilled water, greatly diminish the solvent action of that water upon lead.

(4.) A small quantity—about 15 grains per gallon—of these last mentioned salts exercises almost as great a deterrent action as a comparatively large quantity, about 35 grains per gallon.

(5.) The amount of lead dissolved increases but slightly after the lapse of twenty-four hours, in the presence of these salts which exercise a deterrent action upon the solvent power of water on lead. In the presence of salts which increase this action—notably of nitrates—the amount of lead dissolved increases with the length of time during which the water remains in contact with the lead. I cannot speak with certainty upon this point for a greater length of time than 72 hours.

In these experiments the lead was uniformly clean and bright. Inasmuch as natural waters, even if contaminated with nitrates, usually contain small quantities of soluble carbonates, sulphates, or chlorides, the solvent action of these waters upon leaden pipes and leaden cisterns may, I think, be said to be, under ordinary circumstances, exceedingly small. I would especially draw attention to the experiment

made with water containing 1·4 grains of ammonium nitrate and 42 grains of calcium chloride per gallon; the amount of lead dissolved, after 72 hours being only 0·126 grains per gallon, whereas water containing the same amount of ammonium nitrate, but without the addition of any other salt, dissolved 1·75 grains per gallon, or about fifteen times as much lead in the same time. As a water which contains nitrates very often also contains chlorides this reaction is one of some importance.

Under certain circumstances there can be no doubt that water will dissolve considerable quantities of lead, this is especially the case with water charged with carbon dioxide, and with hot water

Dr. Roscoe has described a case in which a leaden cistern was very quickly acted upon by hot water.*

For the following details regarding the amounts of lead found in various samples of aerated beverages in the manufacture of which leaden apparatus had been employed, I am indebted to my friend, Dr. Milne, of Glasgow.

TABLE B.

Lead found in various samples of aerated beverages.

Description of Liquid.	Quantity of Lead in grains per gallon.
Lemonade	0·20
Do.	0·40
Do.	0·05
Gingerade	0·10
Sodawater	0·60
Do.	0·05

In addition to these numbers I have determined the following, which show the amounts of lead dissolved by distilled water charged with carbon dioxide at the ordinary atmospheric pressure for varying lengths of time, and also the amounts dissolved by the same water on the addition of various salts. The surface of lead exposed measured 2,100 square millimetres.

* Proc. Lit. and Phil. Soc., of Manchester, Vol. xiv., p. 23.

TABLE C.

Lead dissolved by water charged with carbon dioxide at ordinary pressure.

	Mgms. per litre.	Grains per gallon	Lead Dissolved. In mgms. per litre. After 24	48	72	In grains per gallon. 24	48	72 hrs.
Distilled water charged with Carbon Dioxide			3	3	3	0·21	0.21	0·21
The water poured off, more added and again poured off, and finally fresh water containing Carbon Dioxide added ...				None.			None.	
Potassium Carbonate and Ammonium Nitrate	100 and 20	7·0 and 1·4		Merest trace.			Merest trace.	

As the results indicated that water charged with carbon dioxide at the ordinary atmospheric pressure exercises no considerable solvent action upon lead, and moreover that this action ceases on the addition of carbonates; the large amounts of lead found in some of the samples of sodawater examined by Dr. Milne are probably due to the increased solvent action of water containing large quantities of carbon dioxide forced into it under pressure. In order to test the truth of this supposition I have made a few determinations of the amounts of lead dissolved by distilled water, and by the same water containing known quantities of various salts when charged with carbon dioxide at a pressure of several atmospheres.

The apparatus consisted of an ordinary *gasogene* used for making so-called "soda water." From the known capacity of the globe and the weight of sodium carbonate and tartaric acid employed the pressure exerted in the interior of the vessel by the carbon dioxide was calculated as approximately equal to 6 atmospheres.

The surface of lead exposed measured 750 sq. mm.

TABLE D.

Lead dissolved by water charged with carbon dioxide at a pressure of about 6 atmospheres.

Salt.	Mgms. per litre.	Grains per gallon.	Lead Dissolved. In Mgms. per litre. 24 hours.	48 hours.	In Grains per gallon. 24 hours.	48 hours.
Potassium Carbonate..	80	5·60	13·2	32·0	0·924	2·24
Ditto	160	11·20	...	6·0	......	0·42
Calcium Chloride	160	11·20	32	44	2·24	3·08
Ammonium Nitrate	16	1·12	5	...	0·35	......
Ditto	40	2·80	10	35	0·70	2·45
Distilled Water	...	...	14·8	24	1·036	1·68

It appears from these numbers that distilled water charged with carbon dioxide under a pressure of (approximately) 6 atmospheres dissolves five times as much lead as the same water charged with the gas at the ordinary atmospheric pressure: that the presence of a small quantity of ammonium nitrate does not increase the solvent action until after a lapse of 48 hours or so: and that potassium carbonate, when present in somewhat large quantities, exerts a marked deterrent action. The amount of lead dissolved, however, even in the presence of potassium carbonate, is far too large to allow of such a water being drunk with safety.

II. *Action on Copper.*

The experiments under this heading were carried out in a manner similar to that already described. The pieces of copper foil presented a surface of 420 sq. mm. to the action of the various solutions. The results obtained were with one exception negative: no copper was dissolved. The action of the following liquids was examined:—*distilled water*; the same containing *ammonium nitrate* in quantities varying from ·02 grams per litre (=1·4 grains per gallon) to ·408 grams per litre (=28·56 grains per gallon); the same containing *potassium nitrate* in like amounts; the same containing *ammonium sulphate* in quantities varying from ·10 to ·20 grams per litre (=7 to 14 grains per gallon): and also distilled water containing simultaneously *carbonates and nitrates, carbonates and sulphates,* and *chlorides and*

nitrates. The length of time during which the copper was exposed to the action of these solutions varied from 18 to 150 hours. The only liquid which exercised *any* solvent action upon the copper was that containing the large quantity of *28·56 grains per gallon of ammonium nitrate*; this action was manifested only after *150 hours'* contact of the liquid with the copper, the amount of metal which had then passed into solution being equal to *3 milligrams per litre*, or *0·21 grains per gallon.*

The general conclusion to be drawn from these experiments therefore undoubtedly is, that at ordinary temperature neither distilled water nor water containing the salts which commonly occur in drinking waters exercises a solvent action upon copper.

That water charged with carbon dioxide will dissolve copper is apparent from the following figures, which represent the amounts of that metal found by Dr. Milne in various samples of aerated beverages.

TABLE E.

Copper found in various aerated beverages.

Description of liquid.	Quantity of copper in grains per gallon.
Soda water	·084
Potash water	·098
Lemonade	·053
Ginger ale	·053
Potash water	·100
Aerated water	·089
Soda water	·100
Aerated water	·084
Soda water	·036

In order to arrive at some accurate measurements of this solvent action of water containing carbon dioxide upon copper, I prepared a number of solutions charged with that gas at the ordinary atmospheric pressure and placed in each a piece of clean copper foil exposing a surface of 2100 sq. mm. The amount of copper dissolved was estimated by adding

sulphuretted hydrogen and comparing the depth of colour produced with that in a standard liquid: the process is exceedingly accurate and delicate.

TABLE F.

Copper dissolved by water charged with Carbon dioxide at ordinary pressure.

Salt.	Mgms. per Litre.	Grains per Gallon.	Copper dissolved. Mgms. per Litre. 24	48	72	120	Grains per Gallon. 24	48	72	120 hrs.
Potassium Carbonate..	200	14	0·1	...	0·15	0·2	·007	...	·0105	·014
Calcium Chloride	200	14	0·7	...	1·20	1·80	·049	...	·084	·126
Ammonium Nitrate ...	20	1·4	0·3	...	0·60	1·40	·021	...	·042	·098
Ditto........................	40	2·8	0·6	...	0·80	1·40	·042	...	·056	·098
Potassium Carbonate and Ammonium Nitrate ...	100 and 20	7 and 1·4	0·2	...	0·30	1·0	·014	...	·021	·07
Potassium Carbonate and Ammonium Nitrate ...	200 and 40	14 and 2·8	trace	...	trace	0·1	trace	...	trace	·007
Ammonium Nitrate and Calcium Chloride	20 and 200	1·4 and 14	0·6	...	2·4	3·6	·042	...	·168	·252
Distilled Water			0·1	0·3	...	1·0	·007	·021	...	·07

The general conclusions which I would draw from these results are:

(1) Distilled water, charged with carbon dioxide, exercises a notable solvent action upon copper, the amount of metal dissolved increasing with the length of time during which it is exposed to the action of the water.

(2) The salts which have the greatest effect in increasing this action are *chlorides* and *nitrates*, especially the latter: if there are *both* present the action is very largely accelerated.

(3) *Carbonates*, especially when present in large quantities, very materially diminish this solvent action.

(4) If *carbonates and nitrates* are *present together* the solvent action of the latter is much diminished by the presence of the former salts, so much so indeed that if the carbonates be present in proportionately large quantities the solvent action upon the copper almost entirely disappears.

I have also carried out a few experiments with the view

of determining the amounts of copper which are dissolved by water charged with carbon dioxide under a pressure of several atmospheres: the results are subjoined.

The apparatus were the same as that employed in the experiments with lead.

Surface of copper exposed = 2100 sq. mm.

TABLE G.

Copper dissolved by water charged with carbon dioxide at a pressure of about 6 atmospheres.

Salt.	Mgms. per Litre.	Grains per Gallon.	Copper dissolved. Mgms. per Litre 24 hrs.	Mgms. per Litre 48 hrs.	Grains per Gall. 24 hrs.	Grains per Gall. 48 hrs.
Potassium Carbonate ...	40	2·8	1·0	1·2	0·7	·084
Ammonium Nitrate......	16	1·12	...	0·8	...	·056
Ditto	80	5·60	1·2	1·4	·084	·098
Distilled Water...........	...	...	0·4	0·6	·028	·042

Distilled water, charged with carbon dioxide, under a pressure of (approximately) 6 atmospheres, dissolves about three times as much copper as the same water charged at the ordinary atmospheric pressure. Nitrates increase this action and carbonates diminish it.

III. *Influence of house cisterns upon the water supply.*

There appears to be a somewhat wide-spread feeling against the use of cisterns in dwelling-houses, which is, I suppose, chiefly due to the fact that the waste pipe from the cistern is generally in connection with the soil pipe which carries off the drainage of the house.

The hurtful sewer gases may thus readily find their way into the cistern, and so contaminate the water therein stored. On the other hand, however, it may be urged that inasmuch as the water in cisterns is frequently changed there is no great probability that the water actually used for domestic purposes will be, at any rate largely, contaminated by sewer gas. I have attempted to obtain some definite measurements of the amount of contamination

present in cistern waters, in so far as this may be estimated by the chemical processes at present in our possession.

The method which I have adopted consists in measuring the amount of free and of albumenoid ammonia, and the amount of nitrogen existing as nitrates and nitrites: from these data we may deduce, at any rate comparative measurements, of the purity of various waters.

In order to prove conclusively, for my own satisfaction, that if sewer gases be absorbed by water their presence will be indicated by a marked increase in the quantities of ammonia, free and albumenoid, obtained from that water on analysis, I carried out the following preliminary experiment:—

A quantity of distilled water, free from ammonia, was placed in a porcelain basin, which was covered with porous paper, and suspended at a short distance above the liquid in a sewer which received the refuse from a very large area, chiefly occupied by dwelling-houses, in Glasgow. After 96 hours, the free and albumenoid ammonia were estimated with the following results:—

Free ammonia = 0·60 mgms. per litre = parts per million.
Albumenoid „ = 0·54 „ „ = „ „

It is thus evident that the absorption of sewer gases by water causes a marked increase in the quantities of ammonia obtained on analysis.

The method adopted for the estimation of ammonia was the well known one of Wanklyn and Chapman: the method for the estimation of nitrates was that described by Thorpe in the Journal of the Chemical Society for June, 1873. This method consists in evaporating the water, along with a fragment of ignited quicklime, to a small bulk, and then evolving the nitrogen, as ammonia, by the action of zinc, coated with a deposit of spongy copper, at a boiling heat.

In selecting the waters for examination I endeavoured, as far as possible, to obtain typical samples: in this endeavour

I was greatly aided by the kindness of Mr. Macleod, the Sanitary Inspector for Glasgow, who obtained for me samples of waters from various houses situated in the lower parts of the town.

The results are calculated as follows :—

TABLE H.

Ammonia and nitrates found in various samples of water.

	Mgms. per Litre=Parts per Million.											
	i.	ii.	iii.	iv.	v.	vi.	vii.	viii.	xi.	x.	xi.	xii.
Free Ammonia	·005	·085	·023	...	·015	·015	·010	·035	·015	·075	·200	·045
Albumenoid Do. ...	·092	·120	·080	·082	·090	·080	·085	·085	·070	·065	·370	·090
Nitrogen as Nitrates & Nitrites.	·309	·463	...	·321	·360	·20	·258	...	·284	·306	·414	...

These samples were obtained from the following situations. No. 1. *From main pipe.* No. 2. *From cistern in same house, little used.* No. 3. *From cistern in house similar to No. 2, but water generally used.* No. 4. *From pipe leading directly out of the bottom of cistern in well-situated dwelling house.* No. 5. *From cistern in smaller dwelling house.* No. 6. *From small cistern supplying part of a dwelling house only.* No. 7. *From public well supplied by Loch Katrine water contained in a wooden cistern closed at the top.* No. 8. *From cistern situated just under the slates in a house in a lower locality than any of the preceding.* No. 9. *From cistern over water-closet in a dwelling house.* No. 10. *From cistern similar to above.* No. 11. *From the cistern same as No. 9, but after stirring up the muddy deposit at the bottom.* No. 12. *From cistern near the slates in a house where there had been two cases of fever and where the water was complained of.*

Omitting for the present No. 11, it is found that No. 2 sample yields the highest number for free and for albumenoid ammonia, also for nitrates. Now this sample was taken from the cistern of a house in which the pipes have been recently entirely renewed, and in which the pipe leading from the water-closet to the main drain is thoroughly venti-

lated. The water in this cistern is however very rarely used; for all domestic purposes a supply is obtained directly from the main; it would therefore appear that sewer gases are slowly absorbed by water stored in such a cistern. That this absorptive action must take place slowly is evident if we look at the results obtained from the other waters. Although many of these waters were taken from badly situated cisterns, yet in none of them can the influence of sewer gases be distinctly traced. We must therefore conclude that the rapidity with which the water in the cistern has been changed has prevented any appreciable action of the gases upon these waters. There are, it is true, slight variations in the numbers obtained, but in no case do we find a notable increase as compared with water from the main pipe.

The amount of ammonia, &c, obtained from a sample of the slimy matter found at the bottom of one of the cisterns (No. 11) indicates that a great part of the ammonium salts, &c., is concentrated therein; this matter may therefore perhaps exercise a certain beneficial effect upon the water.

The general conclusions which I would draw from these results are:—

(1.) That sewer gases are absorbed by water, but that this absorption takes place slowly.

(2.) That in ordinary house cisterns the water is not contaminated to any great extent with sewer gases, probably because of the short time during which this water is allowed to remain in the cistern, and also perhaps because of the deposition of part of the impurities in the muddy substance which settles at the bottom of the cistern.

The general problem of the influence of the means of supply upon potable waters is a very wide one. I offer these measurements as a contribution towards its solution.

In the course of the discussion which took place after the reading of the above papers several of the members stated that the town's water as received at their houses was sometimes very turbid, and the President said it ought never to be used for drinking purposes without being filtered.

J. G. LYNDE, M. Inst. C.E., F.G.S., said that at his house he had always found the water quite clear and pure, and that although he had a filter it was now never used.

T. H. G. BERREY, Ass. Inst. C.E., superintendent of the Manchester Corporation Water Works, stated, in reply, that there was no reason why the water should be discoloured at the points named, except that it must have been caused by the neglect of the turncock to cleanse the hydrants according to instructions. The general supply throughout the 36 townships supplied by the Manchester Corporation was quite satisfactory, and the water was never rendered turbid except in the case of a very heavy thunderstorm, when the turbid water would sometimes, during the night, get into the service reservoirs from the adjoining brooks before it could be sent down the flood watercourse.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 8th, 1875.

ALFRED BROTHERS, F.R.A.S., in the Chair.

Mr. PERCIVAL exhibited specimens of Bryum Neodamense Itzig [=B. formosum, Wilson MSS.] found by him, in company with Dr. Wood and Mr. T. Rogers, on May 23rd, 1875, fruiting abundantly. Hitherto it had only been found in a barren state in Britain. The locality is near Freshfield, Lancashire.

The true place of this moss is next to B. pseudotriquetrum, common upon the Lancashire mosses.

It was originally found at Birkdale, near Southport, but soon disappeared, owing to draining and building operations; then it was again observed at the Bullrush Slack, by its original discoverers, viz., Dr. Wood, Mr. W. Wilson, and Mr. H. Boswell, but never in a fruiting condition.

Mr. Thos. Rogers exhibited living specimens of the rare Irish Slug, *geomalacus maculosus*, from Lough Corrib, its only known British habitat, and even there its range is exceedingly restricted.

"The Fauna of Cymmeran Bay, Anglesea," by John Plant, F.G.S.—Part 2.

The list of the fishes and mollusca which I read to the Section last April included all the species which had been identified from the collections made in the summer of 1874 and at Easter, 1875.

The present list includes those which have been added by a further examination of the shores and waters along the coast from Rhoscolyn to Aberffraw, of which line Cymmeran Bay forms the centre.

The time devoted to the work extended from the end of July to September, when the weather was rarely disturbed by furious gales, such as prevail on this coast in spring and autumn—so that the new collection possesses none of the varieties which the rude waves of the Atlantic at such times cast upon the rocks and sands.

The species in this list are therefore more likely to be local and permanent than are some included in the former list.

During the summer a fine young seal, *Phoca vitulina*, was caught fast asleep upon the rocks in the bay, and kept in confinement a few days with the intention of sending it to

Belle Vue Gardens, Manchester, but it managed to escape from the room it was kept in, and actually travelled down to the rocks, and got away to sea.

The fishermen say that the seal is well known along the coast, and breeds in the more inaccessible clefts and caverns in the bold sea cliffs, from the North Stack to Rhoscolyn. The Welsh name for The Skerries, the well known rocky islets off Carmels Point, is *Ynysoedd Moelroniaid,* the Isles of Seals. A century back, the seal was very common on these rugged and little frequented coasts.

A few years ago, a shoal of Cetaceans found their way to the head of Cymmeran Bay and were stranded in the strait which divides Holyhead from Anglesea. They proved to be the Bottle-nosed Dolphin, *Tursio truncatus,* of Montagu.

The common Porpoise, *Phocæna communis,* not infrequently is seen and captured when sporting out in the channel.

The additions to the fishes already enumerated, are not many, for it is not easy to obtain specimens of the species that are small and worthless for the table—they readily escape through the nets used for mackerel and herrings.

Specimens of *big sharks* are often seen in the bay by the fishermen, but they give them a wide birth, and I cannot make out the species from the imperfect descriptions given in such an unscientific tongue as is the mixed Welsh and English.

The Picked Dogfish, *Acanthias vulgaris,* will at times get caught in the nets during the night, after making a hearty meal upon the cod and whiting.

The spotted Ray, *Raia maculata,* is often captured by trawling, but it is not used for food, being cut up like its congenor the thornback Ray, as bait for lobsters and crabs.

The Turbot, *Rhombus maximus,* is caught of moderate size.

The Garfish, *Belone vulgaris*, rare.

The Grey Gurnard, *Trigla gurnardus*, is common with the Piper Gurnard all the summer.

The additions to the former list of mollusca made during a long visit have been sixty-two species, making a list of one hundred and forty-one species. It is remarkable that although the sandy shores about Cymmeran and Aberffraw bays are both extensive and level between high and low tidal marks, such well-known common species as *Cardium edule, Turritella communis, Mytilus edulis,* and others, should be excessively scarce, whilst such species as *Cypræa Europœa, Patella pellucida, Tellina fabula, T. tenuis, Cyprina Islandica, Mactra stultorum, Pecten maximus,* and Littorinæ should be fairly abundant, if not quite common.

The small species of *Rissoa Odostomia, Skenea,* and *Mangelia,* occur in great numbers, the sand in places being almost made up of these shells, mixed with Entomostraca, Serpulæ, comminuted shells, and crustacean claws. The varieties of *Rissoa parva, costata* and *striata* are endless, and it would be easy to multiply species of this genus, and also of *Skenea,* if only extremely marked or smooth specimens were regarded.

In my former list are many species which were called rare, as only one or two specimens or even a single valve had been found in 1874, or at Easter of 1875. In some cases this rarity has been proved by not finding additional specimens this summer, as in *Corbula, Psammobia, Dentalium, Turritella* (only two), *Chemnitzia, Trophon,* and *Cylichna.*

Amongst the fresh species there are none that occur abundantly, but *Pecten pusio, Cyprina, Scaphander, Ceratisolen, Crenella, Adeorbis, Kellia,* and *Assiminea* are found in fair numbers between high and low tidal marks. The rock-borers and tunicata have had but cursory attention paid to them at present.

Some species are very local, being only found within a small rocky cove, or else confined to a limited zone on the wide sands either at Aberffraw or at Cymmeran, a few even are restricted to smaller patches of sand in one of the numerous little creeks along the coast.

Additions to the Mollusca.

CEPHALOPODA.

80. Sepia officinalis. The animal is occasionally caught in the dredges, nets, and lines of the fishermen, and the bone or shell is thrown on shore after a gale.
81. Sepiola Atlantica, seen at times in the deeper pools of the rock.
82. Octopus vulgaris. This singular animal has been taken, but it is very seldom kept, as its uncanny ways make it dreaded by the fishermen.
83. Loligo vulgaris, taken in the nets. It is probably common amongst the rocks, as my son saw the inky cloud discharged from one in a small pool as his boat passed over the spot.

ACEPHALA.

FAM. I.—PHOLADIDÆ.

84. Teredo norvagica, portions of the tube found in wreck.
85. Teredo navalis. The tube of this common shipworm is found in old wreck floating into the bay, after months of drifting in the Atlantic or in the Channel.
86. Pholas candida. Specimens are rare; no doubt the absence of limestone rocks along this coast accounts for the rarity of the Pholadidæ.
87. Saxicava rugosa. A few specimens.
88. Mya truncata. A few perfect shells.

FAM. VI.—ANATINIDÆ.

89. Thracia phaseolina. One valve only.

FAM. VII.—SOLENIDÆ.

90. Solen ensis.
91. Solen marginatus.
92. Ceratisolen legumen.

FAM. IX.—TELLINIDÆ.

93. Syndosmya prismatica, one specimen, rare.

FAM. XI.—MACTRIDÆ

94. Lutraria elliptica, many *thick* valves, separated and broken by the gulls.
94*a*. Mactra elliptica, rare.

FAM. XII.—VENERIDÆ.

95. Venus verrucosa, small variety.
96. „ casina.

FAM. XIII.—CYPRINIDÆ.

97. Circe minima, very rare, only one valve.
98. Astarte sp.?

FAM. XIV.—CARDIADÆ.

99. Cardium fasciatum, very rare.

FAM. XVI.—KELLIADÆ.

100. Montacuta ferruginosa, rare.
101. Turtonia minuta, one valve, rare.
102. Kellia rubra.

FAM. XIX.—MYTILIDÆ.

103. Crenella marmorata.

FAM. XX.—ARCADÆ.

104. Nucula decussata, rare, one valve.
105. Pectunculus glycimeris, rare, one valve.

FAM. XXI.—OSTREADÆ.

106. Pecten pusio, fine living shells, dredged.
107. „ tigrinus, rare, one very small valve.
108. „ opercularis, rare.
109. „ niveus, rare.
110. Anomia striata, rare.
111. „ ephippium, many varieties of this species, occur commonly at high water line, living specimens dredged.

PROSOBRANCHIATA.

112. Chiton, sp. These singular mollusca occur pretty commonly but the species are not yet satisfactorily identified.

FAM. XXVIII.—PATELLIDÆ.

113. Pilidium fulvum, rare, one worn specimen.

FAM. XXXIII.—TROCHIDÆ.

114. Trochus lineatus, very fine specimens about the rocks at low water—abundant.
115. Adeorbis subcarinata, a few specimens.

FAM. XXXVII.—LITTORINIDÆ.

116. Littorina saxatilis, one specimen.
117. „ tenebrosa, and varieties.
118. Lacuna pallidula.
119. „ puteolus.
120. „ vincta, and varieties.
121. Assiminea Grayana.
122. Rissoa calathus.
123. „ ventrosa, dead shells
124. Skenea planorbis.
125. „ nitidissima.
126. „ rota.

FAM. XLI.—PYRAMIDELLIDÆ.

127. Eulima polita.
128. Chemnitzia sp., uncertain until other specimens are found for comparison.
129. Odostomia conoidea.
130. „ rissoides.
131. „ unsculpta.
132. „ spiralis.
133. „ sp. uncertain.

FAM. XLV.—MURICIDÆ.

134. Nassa incrassata.
135. Fusus antiquus, brought up alive by dredging.

FAM. XLVI.—CONIDÆ.

136. Mangelia linearis.

Fam. XLVIII.—Bullidæ.

137. Tornatella fasciata, rare.
138. Scaphander lignarius.

TUNICATA.

Fam.—Ascidiatæ.

139. Ascidia mentula. I found a very fine one attached to a piece of slate, at a very low tide: it was nearly six inches long.
140. Ascidia sp. Several other forms have been seen, but not taken for preservation.

Crustacea.—Upon further exploration it has been found that the shrimp is a very rare species all along this coast, but that the prawn, *Palæmon serratus*, is very abundant.

The fishermen firmly believe that the lobsters and crabs are sensitive to coming violent changes in the wind and weather, and maintain that they forsake the neighbourhood of the rocks in the bay and go far out into deep water in the channel 24 to 48 hours before the storm breaks on the coast; for none are ever caught in the ports a day or two before the storm. That fishes leave the shallow waters of the bay for deeper water before a storm, is a fact for which I can myself bear testimony, as well as the fact that on warm quiet days and nights, when the water is oily and without a ripple, the fish frequent the bay in thousands.

The Crustacea, Balanidæ, Lepadidæ, Annelidæ, Echinodermata, Actinozoa, are only at present half ascertained, but a sufficient number of specimens have been obtained to make their further collection important; and these, together with the land and fresh-water shells, will be given in a future list of the fauna of Cymmeran.

Ordinary Meeting, December 14th, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Professor SCHORLEMMER exhibited a sample of peat from lagoons in the Sierra Madre in Mexico. It is very dense and not readily inflammable, giving very little flame, but when once red-hot it burns completely, without requiring much draught, to a perfectly white ash containing much calcium carbonate and a little sodium sulphide, which is derived from glauber salt which the peat contains.

"On Graphic Methods of Solving Practical Problems" by Professor OSBORNE REYNOLDS, M.A.

In the first part of this paper it is pointed out that, when dealing with practical problems by the aid of the graphic method, it is not necessary to break off the operations of drawing, and find numerical values for the quantities represented, in order to perform on them the operations of multiplication and division. For by the aid of a parallel ruler the operations of multiplication and division may be performed graphically with great facility. The only geometrical proposition involved being that of finding a fourth proportional to three distances. When two distances have to be multiplied or divided the one by the other, a third distance is chosen equal to unity, and a fourth proportional found which represents the product or ratio of the first according as unity is the first or third of the given quantities.

The method was illustrated as applied to the determination of areas, centres of gravity, and moments of inertia.

In the second part of the paper a graphic method is described by which the velocity and acceleration of a moving point can be determined when the times at which it

occupies certain positions are known, *i.e.* the curves representing the velocity and acceleration of the point may be drawn from the curve representing the positions of the point. Also a converse method by which the position of a point at any time may be found from the curve representing either its velocity or displacement.

"On Explosions of Fire Damp."

E. W. BINNEY, V.P., F.R.S., said that the fearful loss of life in our coal mines deserved the careful attention of all societies like ours. It ought to be one of the objects of science to endeavour to find out the cause of these explosions and to devise some means to prevent their occurrence or lessen their frequency. No doubt Government Inspection had been of service, and the examination of managers would tend to improve the efficiency of mining officers; but still notwithstanding these improvements the explosions of fire damp are sadly too frequent. The lamentable events which have taken place during the last ten days clearly show that they sometimes occur without any great change in the barometric pressure of the atmosphere, although undoubtedly sudden depressions in a barometer ought to caution miners against emission of gas from the seam of coal and coal wastes, and put the men more on their guard at such times.

It has been stated in this society that certain conditions of the atmosphere quite irrespective of barometric pressure may have something to do with causing the "drag" in the currents of air circulating through a mine, as explosions have frequently occurred during an east wind and a muggy state of the atmosphere, and a vesicular condition of water in the air has been suggested as the probable cause of this lessening of the speed of the air passing through the galleries of mines. Now, careful observations with a good anemometer in the return air course of a mine ought to

determine whether or not such an effect is produced, and thus settle this point by direct experiment.

Another source of accidents at this time of the year has to be taken into consideration. Before Christmas and in cold weather there is often a brisk demand for coal, and both managers and men are in a hurry to increase the output, and under such circumstances probably there may be sometimes not so much care and caution exercised as are necessary for them to use in the dangerous work in which they are engaged.

In the management of a fiery mine, in my opinion,

1. There ought not to be any unventilated wastes.

2. The mixed use of Davy lamps and naked lights should not be permitted where the former are commonly employed.

3. Blasting of coal by gunpowder should not be sanctioned where Davy lamps are in common use.

4. An anemometer under the care of a competent man should be in constant use in order to see that a sufficient current of air is passing through the workings to insure perfect ventilation of the mine..

5. When there are marked indications of fire damp in a mine, shown by a cap on the flame of a lamp, the men engaged in hewing and drawing coal should be removed from the pit until by ventilation the place is cleared of gas and rendered safe for a working collier.

The above precautions may probably cause an increased cost in the getting of coal, but they are necessary for the preservation of human life if such catastrophes as now frequently occur are to be prevented. It is now pretty generally admitted that all explosions of fire damp are caused by there being too little pure air and too much of that gas in a mine.

"Chemical Notes," by M. M. PATTISON MUIR, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

I. *On the Solubility of Potassium Perchlorate in Water.*

Having a small quantity of pure Potassium perchlorate at my disposal, I thought it might be interesting to determine the solubility of this salt in water at different temperatures.

The apparatus employed was similar to that described by Hannay.*

The salt was placed in a small test tube to which a thermometer was strapped, the whole being surrounded with ice or water maintained at the proper temperature.

The following were the results obtained.

A. Temperature 0° C.

Weight of liquid in the bulb 4·722 grms.

Weight of residue on evaporation 0·0333 grms.

Weight of distilled water contained in the bulb at 0°, 4·7575 grms.

Weight of bulb itself, 5·3954 grms.

Hence the specific gravity of an aqueous solution of this salt saturated at 0° equals 1·0005: the percentage of salt in solution is 0·705: and the solubility of the salt is 1 part in 142·9 parts of water.

B. Temperature 25° C.

Weight of liquid in bulb 4·7418 grms.

Weight of residue on evaporation 0·0907 grms.

Other weights as before.

Specific gravity of aqueous solution saturated at 25°, 1·0123.

Percentage of salt in solution, 1·92.

Solubility, 1 part in 52·5 parts of water.

C. Temperature 50° C.

Weight of liquid in bulb 4·798.

Weight of residue on evaporation 0·243.

Specific gravity of aqueous solution saturated at 50°, 1·0181.

Percentage of salt in solution 5·07.

Solubility, 1 part in 15·5 parts of water.

* J. Chem. Soc. [2] xii. 203.

D. Temperature 100° C.

Weight of liquid in bulb 4·9965.

Weight of residue on evaporation 0·7870.

Specific gravity of aqueous solution saturated at 100°, 1·06603.

Percentage of salt in solution 15·76.

Solubility, 1 part in 5·04 of water.

For each rise of 25° the solubility and the percentage of salt in solution increase in round numbers threefold.

I may add that I found Hannay's apparatus exceedingly accurate and serviceable.

II. *On Basic Bismuth Perchlorate.*

If metallic Bismuth be heated with an aqueous solution of perchloric acid it is slowly converted into a white non-crystalline mass. This substance is insoluble in water; when thoroughly washed and dried between folds of blotting paper it presents the appearance of a bulky, pure white powder which it is difficult to obtain equally divided as the particles tend to gather together and form small more or less compact masses. This substance yields the following numbers on analysis:

(*a*)	0·364	grams	gave	0·2675	grams	Bi_2O_3	= 0·2382	grams	Bi.
(*b*)	0·4173	„	„	0·298	„	„	= 0·267	„	„
(*c*)	0·450	„	„	0·3231	„	„	= 0·290	„	„

	Calculated for $BiO.ClO_4$	Found. I.	II.	III.
Bismuth 210	64·52	65·44	63·98	64·44

These numbers agree very well with those required by the formula $BiO.ClO_4$, or it may be written $Bi(ClO_4)_3.Bi_2O_3$.

Basic Bismuth perchlorate is almost perfectly insoluble in water even at 100°; it is very readily dissolved by hydrochloric or nitric acid; less readily by sulphuric acid; at a red heat it is decomposed with formation of bismuth chloride which is slowly volatilised.

III. *On the Amount of Carbon Dioxide in the Air of Sea Coast Places.*

Thorpe (Chem. Soc. J. [2] v. 189) has shown that the air over the ocean contains less carbon dioxide than air over the land, the mean numbers being 3·0 and 4·04 vols. per 10,000 of air respectively.

During the long vacation I interested myself with a few experiments upon the air of the sea coast with a view to determine whether it inclined, as regards carbon dioxide, to sea air or to land air.

The samples of air were collected at *Ardrossan*, a small town on the firth of Clyde, where the river is almost entirely merged in the open sea.

The estimations were conducted in accordance with Pettenkofer's method.

Date, 1875.	Weather.	Place and Time.	Temp.	Barom.	Wind.	Vols. of CO_2 per 10,000 of air.
Aug 2.	Fine but cloudy.	In boat, ½ mile from shore; 12 noon.	16°·5	767mm.	W by S	3·87
„ 4.	Clear, cloudless sky. Sunset.	On shore, 8 p.m.	21°	760mm.	W by N	3·88
„ 14.	Fine, fresh breeze	200 yards from shore, 3 p.m.	21°	760mm.	SW	3·34
„ 18.	Fine, very clear; very heavy rain during preceding night.	On shore, 8.30 a.m.	16°	759mm.	NW	3·40
„ 21.	Fine, very clear; rain during morning.	In boat, ¼ mile from shore, 2.30 p.m.	17°·5	767mm.	NW	3·84
Sept. 3.	Fine, showers during preceding days.	300 yards from shore.	16°	759mm.	NW	4·01

Mean = 3·72 vols. CO_2 per 10,000 of air.

The air of such a place as Ardrossan, although it be situated almost in the open sea, is not therefore influenced by the sea, so far as the carbon dioxide is concerned, but contains almost the same amount of that gas as is found in land air.

Ordinary Meeting, December 28th, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

The following communication from Dr. JOULE, F.R.S., V.P., was read:—

Unsuccessful attempts have recently been made for the purpose of utilizing a modification of the common kite as a means of obtaining a view of the surrounding country. The machine in each instance rose only to fall violently to the ground after remaining in the air a very short time. These trials have brought to my recollection some experiments I made more than six years ago, but of which I did not publish the results, imagining that all such matters must have been thoroughly elucidated by the Chinese, if not by our own more juvenile kite flyers. The usual method of making the skeleton of a kite is to affix a rather slender bow to the top of a standard, tying the extremities of the bow to twine fastened to the bottom of the standard. The steadiness of the kite in the air depends on the fact that the wings yield with the wind. If the bow is too stiff and the surface nearly a plane, instability results. A kite ought to have a convex spherical surface for the wind to impinge upon. Such a surface I readily made by fixing two bows crosswise. The string was attached to a point a little above the centre of the upright bow, and a very light

tail was fastened to the lower end. The kite stood in the air with almost absolute steadiness. I found that by pulling strings fastened to the right and left sides of the horizontal bow, the kite could be made to fly 30° or more from the direction of the wind, and hence that it would be possible to use it in bringing a vessel to windward. One great advantage of such a mode of propulsion over ordinary sails would be that the force, however great, could be applied low down, so as to produce no more careening than that desired by the seaman.

E. W. BINNEY, V.P., F.R.S., said that in the Isle of Man there had been a prevalence of easterly winds throughout the months of October and November, such as he had never experienced during a residence of ten years. This appears to have influenced the migration of swallows. In the beginning of September the chimney swallows and the house martins assembled in great numbers on his buildings on Douglas Head, as they were accustomed to do prior to their annual departure, and disappeared. On the 5th of November, between 10 and 12 a.m., he observed a dozen house martins *(Hirundo urbica)* in front of his house and between it and the sea, busily employed in pursuing their prey. During the summer months the swift and sand martin are frequently seen in the same locality, but seldom the swallow or house martin, and he was inclined to believe that the presence of the latter was due to their having been driven out of their course by the easterly gales.

Ordinary Meeting, January 11th, 1876.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"Note on a Method of Comparing the Tints of Coloured Solutions," by J. BOTTOMLEY, D.Sc.

In the last number of the *Chemical News*, January 7th, is a letter from Mr. Thos. P. Blunt, M.A., proposing a new method of ascertaining the quantities of bodies in solution by colorimetrical experiments. To two cylinders containing equal columns of the fluid to be examined for a certain substance he adds measured quantities of that substance, adding more of it to one cylinder than to the other; he then purposes to shorten the darker column until it corresponds in depth of tint with the other, then from the length of the two columns he calculates the quantity of the substance originally present. At the end of his letter, alluding to the method for shortening the column, he states "The appropriate apparatus mentioned above so far as I am aware is still a thing of the future." Mr. Blunt's suggestion with slight variation may be practically carried out as follows:— Take two cylinders of equal diameter; inside, at the bottom of each, place a white porcelain disk (the flat lid of a crucible will do). In these cylinders let there be equal columns of fluid. In one cylinder, which may be called A, let a tint be produced by a known quantity of the substance sought. Suppose B to contain this substance, and first sup-

pose the tint in B darker than A. Take a white porcelain lid and elevate it or depress it in B until the tint produced is the same as in A, then if the height of the column of fluid above the disk be h, and the total length of the column H, and p the weight of the substance in A, then the weight in B will be $\frac{p\,H}{h}$. If the tint be stronger in A than in B, then the disk must be allowed to sink in A until identity of tint is obtained. It will be convenient to have the cylinders of equal radii. If they are not so, then if R be the radius of B and r the radius of A, the quantity of the substance sought in B will be $\frac{R^2}{r^2}\,\frac{H}{p}\,p$. Instead of supporting the string attached to the disk by the hand it would be more convenient for it to pass over a pulley and have a counterpoise at the other extremity

"On Explosions of Fire Damp," by ROBERT RAWSON, Esq., Hon. Member of the Society.

Mr. Binney has done wisely in calling the attention of the Society to the great loss of life in our coalmines by the explosion of light carburetted hydrogen. Surely the application of science may do a little good, even if its powers are limited to pointing out the advantages of order and obedience while contending with a foe so subtle and powerful as is carburetted hydrogen, and which is unfortunately so plentiful in many of the coalmines. My object now is to supplement the excellent rules recommended by Mr. Binney with a few observations which struck me in reading Mr. Binney's paper on this subject. Fire damp may be regarded as an enemy possessing terrible powers—and coalminers on the other hand may be regarded as an army whose mission

is to filch the black diamonds from the grasp of this powerful enemy with the least possible expenditure of life and force. In my opinion a little of military obedience and tactics might be introduced with advantage into the struggle maintained between the coalminer and the fire damp. It is well known that the safety of an army, when before an enemy, is much improved by keeping a good look out at a considerable distance and in every possible direction. Vedettes are usually stationed on the outposts of an army for the express purpose of watching the movements of the enemy, and to give timely notice of his approach. By this means the evils which frequently follow a surprise are considerably diminished, and sufficient time is given for the army to retreat or otherwise with advantage if the enemy is too strong. Many illustrations of this simple and obvious principle may be found in the common affairs of every-day life, but the above is sufficient to show the force of my argument in reference to the dangers met with in coalmines. Why the army of coalminers should be deprived of a system which affords so many obvious advantages is a question which should receive immediate and serious consideration from every lover of humanity. A system of vedettes should therefore be at once adopted in coalmines to watch the movements and strength of the fire damp.

1. A vedette, armed with ample knowledge of the properties of fire damp, should be placed at the various outposts of the mine where the enemy is likely to appear. His duty should be to watch, constantly and carefully, over the safety of the miners, and not allow them to remain at work until the enemy is upon them.

2. Miners should not be allowed to work where it is

unsafe to fire a shot or expose a naked light. For men to work several hours in an atmosphere highly charged with light carburetted hydrogen may be consistent with their desires to maintain their families in comfort and independence, but is hardly consistent with the wish of a great nation for the welfare of its mining population.

3. Unventilated wastes are magazines of dynamite, and according to Mr. Binney should be, if possible, avoided in coalmines.

4. System, watchfulness, and complete knowledge of the enemy's position and force, will no doubt diminish the probability of the happening of events which too often overtake the miners by surprise and fatal consequences.

5. A more palpable and delicate test of the presence and strength of fire damp should be supplied than the flame of a candle or Davy lamp.

I may add in conclusion my conviction, that the increased price of coal incident to the changes here alluded to would be cheerfully borne by the public, who would rejoice to see the adoption, at any reasonable cost, of a system which promises to reduce the fatal casualties in coalmines.

Ordinary Meeting, January 25th, 1876.

E. W. Binney, F.R.S., F.G.S., Vice-President, in the Chair.

"Stannic Arsenate," by William Carleton Williams, F.C.S., Demonstrator in the Chemical Laboratory of the Owens College.

A mixture of moderately concentrated aqueous solutions of stannic chloride and arsenic acid gradually thickens on standing, and after the lapse of two or three weeks solidifies, forming a transparent colourless noncrystalline mass. In order to ascertain the composition of this substance I subjected a considerable quantity of it to dialysis; hydrochloric acid and the excess of arsenic acid or stannic chloride passed through the dialyser, leaving a gelatinous residue of pure stannic arsenate. This jelly is heavier than water, it floats in a liquid having a specific gravity 1·135. Strong acids, and solutions of caustic potash or soda, dissolve it readily. In water it dissolves very slowly. From this aqueous solution certain reagents reprecipitate the arsenate of tin as a gelatinous mass, identical in its appearance, properties, and composition with the original jelly. These reagents are hydrochloric, nitric, and sulphuric acids, the chlorides of barium, calcium, ammonium, and iron, also silver nitrate and potassium iodide. Alcohol, acetic acid, sodium phosphate, mercuric chloride, and the carbonates of sodium, potassium, and ammonium, do not produce any change.

This substance contains a large amount of water, the greater part of which is expelled at a temperature of 100° C. A small quantity however is retained most pertinaciously, and is not driven off at 200°. Below a dull red heat decomposition takes place and fumes of arsenious oxide escape.

51·9 grm. dried at 100° C. left 1·9 grm. residue. Loss of water = 50 grm. or 96·3 per cent.

The residue somewhat resembles gum arabic in appear-

ance. It is soluble in strong hydrochloric acid and in aqua regia, but is insoluble in water, nitric and sulphuric acids.

Analysis of the residue gave the following results:

Wt. tkn.	gave		%	Avge.
0·3897 grm.	...0·1696 grm.	SnO_2	= 34.4Sn	34.46 Sn
0·8214 „	...0·359 „	„	= 34·52 „	
0·3013 „	...0·2395 „	$MgNH_4AsO_4H_2O$	= 29·96As	29·82 As
0·5228 „	...0·397 „	„	= 29·69 „	
0·7842 „	...0·0801 „	H_2O	= 10·21H_2O	10·91 H_2O
0·852 „	...0·099 „	„	= 11·62 „	

The simplest formula agreeing with these results is

$$Sn_3(AsO_4)_4 6H_2O.$$

	As	Sn	O	H_2O
Found	29·82	34·46	—	10.91
Calculated	29·46	34·76	25·18	10·6

Professor REYNOLDS, M.A., exhibited and explained the action of a Geissler's Light Mill.

Ordinary Meeting, February 8th, 1876.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. W. A. Cunningham and Mr. W. Brockbank were appointed Auditors of the Treasurer's Accounts.

Professor C. SCHORLEMMER, F.R.S., read the following communication, which he received a short time ago from Professor SADTLER, of the University of Pennsylvania:—

"Since I heard from you I have been analysing some of the natural gases from the gas wells in Butler County, Pennsylvania, in the midst of the oil region.

"This gas has been brought in lines of piping eighteen miles to Pittsburg, and is used with great success as fuel in the rolling mills there, so the Geological Survey sent me to collect and analyse it.

"You will remember that Fouqué (*Compt. Rend.* 67,1054) analysed five of these natural gases from widely different localities and found them to be mixtures of the lower members of the paraffin series.

"I find free Hydrogen, Marsh Gas CH_4, Ethyl Hydride C_2H_6, and a trace of Butyl Hydride C_4H_{10}, with some little Carbonic Acid, in the two that I have already analysed. The hydrides of ethyl and butyl I collected qualitatively at the wells by passing the gas through absolute alcohol.

"The occurence of free hydrogen was an unexpected result, as Fouqué found none in any of the gases analysed by him; but its presence is proved by his own equations given for a mixture of the paraffin series."

Professor Schorlemmer added, that Dr. E. Ronalds, of Edinburgh, examined, in 1865 (*Journ. Chem. Soc.* 18, 54) the most volatile constituents of crude American petroleum, and found them to consist of homologues of marsh gas. Marsh gas itself was not present; but in conclusion he says: there appears to be little doubt that marsh gas and, perhaps, even free hydrogen will be found among the gases which are evolved with the oil at the springs.

"Notice of a recent discovery of a prehistoric burial place near Colombier in Switzerland," by WILLIAM E. A. AXON, M.R.S.L., &c.

The *Journal des Débats* of Feb. 1st, 1876, cites from a Swiss paper a notice of an interesting archæological discovery in Switzerland. In sinking the foundations for a building now in course of construction on the border of the lake between Colombier and Auvernier, the workmen came upon some large pieces of stone each about one mètre broad and one mètre 50 centimètres long. These covered a series of cavities formed of flagstones surrounding the opening, which was filled with earth, pebbles, and gravel. The stones are blocks of various sorts of Alpine granite, evidently

fashioned by human labour at a very remote period. In one of the cavities were found fifteen skeletons, one of them being that of a child. The discovery of a bronze ring would appear to indicate that these individuals belonged to the bronze age, but there were also found a stone axe (nephrite), and bears' teeth pierced to form necklaces. The first grave was explored on the 24th January, and further examination was in progress.

Mr. BROCKBANK, F.G.S., exhibited a large collection of granites from the Ravenglass district, and from Criffel, which he had got together with a view to proving the origin of the large granite boulders recently found in the glacial clay or till of this district.

The fine boulder recently placed in this Queen's Park, and which was found in the railway cutting through the drift clay at Collyhurst, proves to be of Ravenglass granite, such as is found in Eskdale, about four miles above Ravenglass. It is a very fine example of a glaciated boulder, being polished and scratched on several sides, and very little weathered, so that it is pretty much in the condition in which it was when deposited in the clay.

Two large boulders were recently found in the foundations for the corporation buildings near the Manchester Exchange. One of these is of Ravenglass granite, of a deep red colour, such as is now found in the Muncaster Fell quarries about two miles from Ravenglass.

The other is a most interesting specimen differing greatly in appearence from the Queen's Park boulder, in being much weathered, and presenting no worn surfaces except one of its sides, and no ice markings. This may partly be accounted for by its having been found in the gravel, but the writer believed it to point to other glacial conditions than those which had obtained with the Collyhurst boulder. This granite appears to be identical with that now worked

in the Spy Crag quarry, near Dalbeattie, in the Criffel district, and if so it affords a most interesting evidence of the northernly origin of our drift current.

The red granite boulder which for many years stood at the end of the watering trough at Rooden-lane, and which now lies in the grounds of Mr. H. M. Ormerod, at Cheetham Hill, is of the red granite of Muncaster Fell, near Ravenglass.

It had been asserted by Professor Dawkins that the Queen's Park boulder was of Shap granite, but the writer is quite certain that this is not the fact, and he has never found any of the Shap Fell granites in the boulder clays of South Lancashire.

E. W. BINNEY, V.P., F.R.S., said, since the publication of his paper on the Drift Deposits of Manchester in Vol. VIII. (second series) of the Society's Memoirs, attention had been directed to the preservation of large boulder stones. The date of that memoir was 1847, and in it was described and figured the fine block of grey granite now placed in Peel Park. It was found in the till or brick clay at Park Place, Higher Broughton, just below the house then occupied by Mr. W. Sale, and remained on the road side till about the beginning of 1850, when the owners of the land were digging a hole to bury the stone, and thus get it out of the way. He wrote a letter to the Editor of the *Manchester Guardian*, which appeared in that paper of the 13th February, 1850, suggesting that the Peel Park Committee should fetch the stone and place it in their park. His appeal was responded to without loss of time, and it now is placed at the entrance to the park. He was glad to find that the preservation of this specimen had been of service in saving other large boulder stones from the rite of burial. He had himself seen the fine specimen of red granite placed in the Park at Macclesfield, and the large block of hard green stone, weighing nearly 20 tons, now preserved in the Oldham Park. It

was very desirable that when any more specimens are met with they should be preserved in the public parks, or some other suitable places for the inspection of the public. Lancashire and Cheshire contain many large boulders, and it is to be wished that they should not be buried near to where they are found, so as to get rid of them. In his remarks on the building stones of Manchester in 1856, Vol. I., p. 194 of the Proceedings of the Society, he stated: "By the facilities which railways now afford one might have expected that some of the beautiful syenite of Shap, containing large crystals of felspar or the grey syenite of Bootle and Ravenglass would have made their appearance in Manchester, but to my knowledge none of them have been used. It is possible they may not have been known to our architects." He was glad to say that the former stone had been introduced into the buildings of our city, and, when polished, had a beautiful appearance. It was a granite of very marked character, and could not easily be mistaken. After a search of forty years he had never found a specimen of it in the drift near Manchester, and when he heard from the newspapers that a block had been discovered at Collyhurst, and placed in Queen's Park, he went to look at it, but that stone was certainly not Shap granite, a rock which he had several times carefully examined in the quarry at Wastedale Head. It was a grey granite, and it was difficult to speak with certainty whether it came from Ravenglass, Dalbeattie, or the Isle of Man, as specimens from those places are much alike, and it would require to be carefully analyzed by a chemist before it could be identified.

"On the formation of Azurite from Malachite," by CHARLES A. BURGHARDT, Ph.D.

Two years ago I placed a specimen of rather poor Malachite (from Alderley Edge) on a rockery in my garden where it would be exposed to the action of the weather, in

order to ascertain whether any change would take place in its composition. Last December I examined the specimen and found it had been acted upon by the weather to a considerable extent, as specks of a dark blue mineral were distributed here and there upon its surface. This dark blue mineral proved to be Azurite. Messrs. Wibel and Tüngel (Dent. Chem. Geo. Berichte IV. 138), in a short paper on the formation of Azurite state that it is formed from Malachite by the absorption of carbonic acid and elimination of water, as Azurite contains a much larger percentage of carbonic acid than Malachite. This statement still remains to be proved, but it is scarcely probable that the moderate heat of our last two summers was sufficient to cause a loss of water in the Malachite specimen. At some future time I shall endeavour to ascertain the real cause of the formation of Azurite.

"On a Direct-Vision Spectroscope of great Dispersive Power," by ARTHUR SCHUSTER, Ph.D.

This instrument is made by Mr. Adam Hilger of London. The following are its chief advantages:—

1. The compound prism has a very great dispersive power. The nickel line between the two sodium lines is easily seen in the solar spectrum.

2. The cross wire is replaced by a very fine slit which can be illuminated from above to any degree of intensity.

3. The slit is moveable by means of a very fine micrometer screw; the position of the slit can be read off to within 0·0001 inch.

The measurement is made by bringing the line to be measured against the bright slit which comes down from the top to the middle of the field. The position of the lines can be easily measured to within the fifth part of the distance between the sodium lines.

"On a New Absorptiometer," by ARTHUR SCHUSTER, Ph.D.

In some recent researches Professor Vogel found that the relative intensity of the red and blue part of the solar spectrum was subject to great changes. While working with the spectroscope at considerable heights on the southern slope of the Western Himalayas, I was struck by the same fact. The instrument which I have now the honour to exhibit before the Society is constructed in order to measure the relative intensity of the red and blue light in the solar or any other spectrum, by comparing the intensity of each ray with that given out by a standard lamp. The photometric principle involved in the measurement is that first used by Professor Zöllner. The intensity of a certain part of the spectrum is brought to the same intensity as that of the standard light by a system of Nicol's prisms. Professor Zöllner only compared the whole intensity of two sources of light and did not investigate the relative intensity of the different colours. D. Glau constructed another apparatus by which he could measure the relative intensity of different colours, but his instrument was constructed for an entirely different object, and is not suitable for the purpose for which the present instrument is made.

The instrument, which I have called absorptiometer, because it is intended chiefly for the determination of the absorption of light taking place in our atmosphere, consists of a table similar to that of a goniometer table, but being able to turn round on a horizontal axis so as to give it any inclination to a horizontal surface. The telescope of the goniometer is replaced by a direct-vision spectroscope. Opposite the spectroscope a tube is fixed to the table containing two Nicol's prisms. One of the prisms is fixed, the other can be turned, and its azimuth read off on a graduated circle. The standard light is placed behind its tube. The intensity of the light falling unto the slit of the spectroscope is $\frac{A}{2} \sin\alpha$, where α is the angle between two of the principal planes of the two Nicol prisms, and A the

intensity of the light which would fall into the slit of the spectroscope if the Nicol's were removed.

A plane parallel piece of glass, acting as mirror, is fixed unto the small table, the centre of which coincides with the centre of the large goniometer table. The parallel sides can be adjusted by means of 3 screws until they are vertical. This mirror reaches to such a height that the horizontal plane laid through the top of the plate would bisect the tube containing the two Nicols.

The light which is to be examined falls through a tube containing one Nicol, and is reflected by means of the plane parallel mirror into the lower half of the spectroscope. If the ray of light is reflected at the angle of polarization the intensity of this light can be reduced to nothing by means of the rotation of the Nicol.

On placing the standard light in front of the tube containing the two Nicols and allowing the light which is to be examined to be reflected into the spectroscope on the mirror through the tube containing one Nicol, the mirror being placed at the angle of polarization, we observe in the spectroscope the two spectra one above the other, and by turning the Nicols we can reduce the intensity of the brighter light to that of the weaker for any colour we like. The positions of the Nicols will enable us to find the relative intensity of the two lights for the different colours.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 6th, 1875.

CHARLES BAILEY, Esq., Vice-President of the Section, in the Chair.

Mr. SIDEBOTHAM, F.R.A.S., sent for exhibition some sand from a river far inland of New Guinea, containing particles of gold, magnetic and non-magnetic iron, foraminiferæ, silicified fragments of echini, and shells.

Mr. J. COSMO MELVILL exhibited two specimens of the Spurge Hawk Moth (Deilephila Euphorbiæ); said to have been captured in the larval state at Ecclesbourne, near Hastings, feeding in all probability on Euphorbia Amygdaloides, as he subsequently visited the spot and could see no trace of any other Spurge.

January 17th, 1876.

JOHN BARROW, Esq., in the Chair.

Mr. SIDEBOTHAM, F.R.A.S., exhibited a magnified drawing and specimens of lymexylon navale from Dunham Park, and read a short paper on the life history of the insect, which he and Mr. Chappell had studied since its discovery in Dunham Park in 1872. Previously to that time only one authentic British specimen was known; this was found at Windsor nearly fifty years ago.

The species is very abundant in some of the old oak trees in Dunham Park, but specimens are difficult to capture, as they fly about the tops of the trees, and to obtain them a net attached to a very long pole is required.

Mr. SIDEBOTHAM also read a paper on Psammodius Sulcicollis, and exhibited specimens taken at Southport in 1875. This has been considered a very rare species, but its habits being now known, it is easy to obtain a comparative abundance. It buries itself in the dry sand at the foot of the sandhills during the day, coming out about 5 p.m.

Mr. PLANT exhibited various objects of interest, including a longicorn Beetle (Astinomus ædilis) from a coal mine near Manchester; also cases of a N. American caddis worm (Phryganea sp.) much resembling a mollusk of the genus Valvata, and once named by Lea Valvata arenicola.

Ordinary Meeting, February 22nd, 1876.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"Notes on a Collection of Apparatus employed by Dr. Dalton in his Researches, which is about to be exhibited (by the Council of the Literary and Philosophical Society of Manchester) at the Loan Exhibition of Scientific Apparatus at South Kensington," by Professor ROSCOE, F.R.S.

The apparatus employed by John Dalton in his classical researches, whether physical or chemical, was of the simplest and even of the rudest character. Most of it was made with his own hands, and that which is to be exhibited has been chosen as illustrating this fact, and as indicating the genius which with so insignificant and incomplete an experimental equipment was able to produce such great results. The Society has in its possession a large quantity of apparatus used by Dalton, most of which however consists of electrical apparatus, models of mechanical powers, models of steam engines, air pumps, a Gregorian telescope, and other apparatus of a similar kind, which was either bought or presented to him. It has not been thought necessary to exhibit these, but rather to show the home-made apparatus with which Dalton obtained his most remarkable results.

I. *Meteorological and Physical Apparatus made and used by Dr. Dalton.*

Throughout his life Dalton devoted much time and attention to the study of meteorology; indeed his first work, published in 1793, was entitled "Meteorological Observations and Essays," and his last paper, printed in 1842,*

* *Vide* Life of Dalton by Dr. Henry, published by the Cavendish Society; Memoir of Dr. Dalton and the History of the Atomic Theory, published in the Memoirs of the Literary and Philosophical Society of Manchester, 2nd Series, Vol. 1; Dr. Lonsdale's Life of Dalton, Longmans, 1874.

(Mem. Lit. and Phil. Soc. VI. 617), consists of auroral observations. Hence the first of Dalton's apparatus which claims attention are the meteorological instruments.

No. 1 is Dalton's mountain barometer, with accompanying thermometer, made for him by the late Mr. Lawrence Buchan, a member of the Society. The barometer is enclosed in a wooden case which Dalton was accustomed to carry in his hand.

Several home-made barometers used by Dalton in his observations are in possession of the Society. They are all of them filled, and the scales prepared, by Dalton himself, and are simple siphon tubes with a bulb blown on at the bottom to serve as a mercury reservoir. These are attached to plain pieces of deal upon the upper part of which the paper scale is pasted. One of these, which has probably also served for tension experiments (No. 2), has been placed in the collection.

Many of the thermometers appear also to have been home-made. No. 3 is a mercurial thermometer evidently made and graduated by Dr. Dalton, and marked with his initials, J. D. The freezing point of this thermometer was tested recently by Mr. Baxendell, who found that it had not altered since the instrument was graduated. Another (No. 4) is of the same kind and bears the date 1823; No. 5 is a third mercurial thermometer with long stem and wooden scale; No. 6 is an alcohol thermometer with wooden scale; and No. 7 a registering maximum and minimum thermometer employed by Dalton, maker's name J. Ronchetti, 29, Balloon-street, Manchester.

II. *Apparatus Constructed and Used by Dalton in his Researches.*

(1) "On the constitution of mixed gases," (2) "On the force of steam or vapour from water or other liquids at different temperatures both in a Torricellian vacuum and

in air," (3) "On evaporation," and (4) "On the expansion of gases by heat."*

No. 8 is an apparatus used for the determination of the tension of volatile liquids at low temperatures; it consists of a siphon tube, at the upper end of which is a scale in inches in Dalton's handwriting. He describes it thus:

"I took a barometer tube 45 inches in length, and having sealed it hermetically at one end, bent it into a siphon shape, making the legs parallel, the end that was closed being 9 inches long, the other 36 inches. I then conveyed 2 or 3 drops of ether to the end of the closed leg and filled the rest of the tube with mercury except about 10 inches at the open end. This done, I immersed the whole of the short leg containing the ether into a tall glass containing hot water."

No. 9 is a smaller tube containing another liquid, also having a graduated scale written on paper and attached to the tube. Nos. 10, 11, 12, 13, 14, are tubes used by Dalton for measuring the tension of vapour from water and other liquids at higher temperatures than their boiling points, both in a vacuum and air. No. 15 is a tube used by Dalton for measuring the tension of the vapour of bisulphide of carbon, labelled "Sulphuret carb.," with a paper scale in Dalton's handwriting, and a cork showing that the upper portion of the tube containing the bisulphide of carbon could be heated in a water bath to various temperatures. No. 16 is a manometer tube, fixed into a board, divided and numbered by Dalton. No. 17 is an apparatus used by Dalton for the determination of the tension of the vapour of ether, and is interesting as being the instrument by means of which Dalton arrived at one of his most important experimental laws. It is described as follows (p. 564):—

* Experimental essays on the above subjects, by John Dalton, read October 2nd, 16th, and 30th, 1801, and published in the 1st series, vol. 5, part 2, of the Memoirs of the Literary and Philosophical Society of Manchester.

"The ether I used boiled in the open air at 102°. I filled a barometer tube with mercury moistened by agitation in ether; after a few minutes a portion of the ether rose to the top of the mercurial column, and the height of the column became stationary. When the whole had acquired the temperature of the room (62°) the mercury stood at 17·00 inches, the barometer being at the same time 29·75 inches. Hence the force of the vapour from ether at 62° is equal to 12·74 of aqueous vapour at 172° temperature, which are 40° from the respective boiling points of the liquids."

This is generally known as Dalton's law of tensions, since shown by Regnault not to be rigorously true.

No. 18 is a wet and dry bulb mercurial thermometer made by H. H. Watson, of Bolton.

III. *Apparatus for Measuring Gases, and for determining the Solubility of Gases in Water.*

No. 19 is an apparatus with a graduated tube, probably used by Dalton for the determination of the laws regulating "the absorption of gases by water and other liquids," read October 21st, 1803*. No. 20 is a graduated glass tube attached to a bottle of indiarubber, also probably used in his researches on the absorption of gases by water. No. 21, No. 22, are divided endiometer tubes, employed by Dalton for measuring the volumes of gases. No. 23 is a spark eudiometer; Nos. 24, 25, 26 are glass tubes, pipettes, and funnels graduated by Dr. Dalton and used by him for measuring gases; No. 27 is a graduated glass bell-jar, used for measuring gases; No. 28 is a phial, with graduated tube attached by cement, for collecting and measuring gases; Nos. 29, 30 are stoppered phials with the bottoms cut off, used as gas jars for collecting and measuring gases; No. 31 is a thousand grains specific gravity bottle, with its counterpoise of lead stamped "175" and paper labelled "bottle

* "Manchester Memoirs." 2nd Series. Vol. 1.

balance;" No. 32 is a pipette; No. 33 square bottle of thin glass, fitted with brass caps, and probably used for the determination of the specific gravities of gases; No. 34 is an earthenware cup, used by Dalton as a mercury-trough, and containing a small phial with mercury; Nos. 35, 36 are bulb-tubes, with graduated scales which may have served for the determination of the coefficients of expansion of gases; No. 37 is a Florence flask with cork and valve for determining the specific gravity of gases; No. 38 is a glass alembic.

IV. *Weights, Balances, Apparatus, Reagents and Specimens used by Dalton.*

No. 39, eleven phials, containing creosote, iodine, amalgam of bismuth and mercury, quercitron bark, grana sylvestra cochineal, and other substances, labelled in Dalton's handwriting. No. 40, three divided blocks, used by Dalton for the illustration of his lectures; these are not, however, the balls an inch in diameter (referred to in his latest memoir on the "Analysis of Sugar") which he employed occasionally in his lectures, as illustrating his newly-discovered laws of combination and the atomic theory; these appear, unfortunately, to be no longer in existence. No. 41 is a common pair of scales used by Dalton; No. 42, a pair of apothecary's scales and weights employed by Dalton, with a paper of weights made of wire, labelled in his handwriting, "100th grains." No. 43 is a box of weights used by Dalton, and containing a pill box labelled "Platina," another pill box labelled "Hund," and containing 100th of grains, and another wooden box containing brass gramme weights, labelled "Weights, French;" the other ordinary weights are of lead. No. 44 is Dalton's pocket balance, consisting of a small pair of apothecaries' scales, with beam about 4 inches long, and having the pans attached by common string; it is contained in a tin case for the pocket.

No. 45 is a penholder used by Dalton. No. 46, leaden grain weights made by Dalton from sheet lead, and stamped in numbers by him; No. 47, iron punches used by Dalton for this purpose. No. 48, a glass lens, wrapped in a piece of paper labelled, in Dalton's writing, "Sun's focus 4·2 inches." No. 49 is a paper containing "10th of grains," made by Dr. Dalton of iron wire. The paper in which these are wrapped is part of a note from one of Dr. Dalton's pupils (as is well known he lived by teaching mathematics at half-a-crown per lesson), in which the writer presents his "compliments to Mr. Dalton, and is sorry that he will not be able to wait upon him to-day, as he is going to Liverpool with a few friends who are trying the Railway for the first time. Mr. D. may fully expect him on Monday at the usual time." No. 50 are bottles of tin, earthenware, and silver, some of them being common penny pot ink bottles. Each has a thermometer tube cemented into the neck of the bottle, and these tubes are provided with paper scales. These were used by Dalton probably for experiments on radiant heat. No. 51 is a manometer tube used by Dalton; it consists of a tin vessel attached on either side to leaden tubing, and having a thermometer-tube closed at the upper end, and provided with a divided scale, fixed into the upper portion of the tin vessel. No. 52, Dalton's balance, made by Accum, and capable of arrangement as hydrostatic balance, with weights and counterpoises.

The following letter from Mr. ARTHUR WM. WATERS, dated Naples, February 9th, 1876, was read by Mr. BAXENDELL:—

Having expected to return ere this, I thought that, in the conversational hour before papers are read, I should have been able to say a few words on the Aquarium at Naples, where I have been working for six weeks, the British Association Committee for the establishment of zoological

stations, having granted me their table; but having, for the purpose of getting further acquainted with the fauna of the Mediterranean, become connected with the institution for a short time, I shall not be in England until after the last meeting of the session, and so send a letter that you may read, if you see fit, when there is a dearth of subjects for conversation.

The zoological station is connected with an aquarium which is more generally known, but is the least important part of the institution, and it was for the sake of the former that the undertaking was started and has been carried on by Dr. Dohrn. There are, in rooms above the aquarium, tables for twenty-four to work, and the greater part of these have been taken up by different governments, but some by other institutions—as the British Association and the University of Cambridge, and Dr. Dohrn is anxious the whole number should be taken, in order to place the station in a satisfactory position.

As the full advantage of such stations are not known to all, a word or two on the opportunities for scientific utility may perhaps have an interest for some of my friends of the Manchester Literary and Philosophical Society.

The station has been established by Dr. Dohrn two years, when he built, as I have before said, an aquarium, together with convenient rooms for study, library, and museum. As the most important direction of zoological research is at the present moment embryology and the study of development, it is natural that it should be principally used for advancing science in this direction, and, as I shall show, in no branch are the advantages more felt; and although this, I believe, was fully appreciated by Dr. Dohrn, when he determined to erect an aquarium at Naples, it was not merely with the idea of advancing zoology in this branch, but that naturalists should have the opportunity of using it for any purpose which they found might forward the branch of science they

had taken up. The greater number who have used the tables here have followed up the embryology of some group or species, but others have worked at the anatomy either of an organ or, on the other hand, of a class or species. I found that it presented great advantage for studying systematically a group, and in the six weeks during which the British Association granted me their table, I have been able to make a large collection of Bryozoa, the determination of which I have not been able to complete, as I cannot obtain some of the most important literature on the subject here; but I may say that I have over one hundred species and expect to find it not under one hundred and fifty, and probably it will be the largest number obtained from any one locality. These I have collected for the purpose of comparison with my collection of Bryozoa from various periods of the tertian formation.

For the study of embryology it is necessary that a very large nnmber of animals should be collected, and that often continuously for a long time; and this is a difficult and expensive thing for a naturalist, especially in a foreign town, but as the station has two fishermen, with a very large knowledge of the animals, who are out every day with instructions to bring such animals as are wanted at the time, and as the fishermen in the whole neighbourhood are encouraged to bring anything rare or that is wanted at the time, it will be clear that for such studies or other anatomical work the advantages are very great, and I have found the same advantage of having the opportunity of constantly overhauling a considerable amount of material for the systematic study of any group of smaller animals. **Each** worker has a good-sized tank and two smaller ones for keeping anything he requires alive. Besides having the animals or plants collected which each is studying, he has on the working table all the reagents which are ordinarily required, besides glass vessels, drawing materials, and cer-

tain simple apparatus, so that the naturalist on his arrival may find at once what he requires for his studies, and can directly set to work; and further, finds a very considerable library of zoological works, which is especially rich in the department of embryology. This was for a large part Dr. Dohrn's private library, but by presents from authors and publishers it has now become a very important library, numbering about three thousand volumes, but being comparatively very recent it is not a matter of surprise that with systematical works it is but very poorly supplied, but in time, by gifts and purchases, it will no doubt possess the most important for determining the fauna and flora occurring here. I should strongly advise any naturalist who intends to study to previously obtain the catalogue, that he may know what books that he is in the habit of using he had better bring with him.

Another very important feature is collecting and preserving the animals brought, so that there is a stock of certain animals in alcohol always ready for anyone who may require it, and supplies are constantly being sent to all parts of Europe to those who require the material for study, and are sent at about cost price. At the present moment there is an application from a neighbouring Literary and Philosophical Society for a supply of Amphioscus in various preservative solutions. A few animals have been determined by specialists who have worked here, and this will form a nucleus of a museum of the fauna and flora of the Bay of Naples. The specimens as yet determined in this manner are too few to have much importance, but when the collection is more considerable it will be very useful, as any one sending for specific specimens can feel more security in the determination when this is necessary.

Besides the knowledge of the fauna and flora, which can be gained by the material brought daily by the fishermen, those who are working here have the opportunity from

time to time to go out dredging; and it is to be hoped that a commencement may be made this spring of systematically dredging in the Bay, so that gradually a very complete knowledge may be gained of the nature of the Bay and the condition of depth and soil, with reference to the various animals found; but this can be carried out much more satisfactorily when the station has the steamer which has recently been presented by the Berlin Academy for the purpose of dredging.

As I indicated above, the expenses of this station are partly supplied by different governments and institutions who pay £75 per annum for a table; in some cases two governments have jointly taken a table, and the tables are granted to those who wish to proceed to Naples for study. At present, England has but two, *i.e.*, the Cambridge University and the British Association; but probably Oxford will soon take one, and why should not Owens join with the other Northern Universities, or with London, to take one?

"On Glacial Action in the Valley of the Wear, etc," by Professor T. S. ALDIS, M.A.

The coal workings in the county of Durham have revealed the existence of a great depressed trench excavated in the coal measures, and partly filled in with drift. The part best known stretches from Durham to Newcastle in nearly a straight line, the Team Valley being the upper visible portion of it, the lower part being filled in for nearly 200 feet.

There is, I believe, no doubt that this "Wash" is the old valley of the Wear.

The glaciers filled up this trench to such an extent that the river, when set free again, often failed to find its former valley—in fact was altogether thrown to the east at Chester-le-Street, entering the sea by a post-glacial valley, though possibly guided in forming it by a dene or glen previously existing.

This diversion of rivers by glacial action is referred to by Geikie, in his "Great Ice Age," but the Wear gives better instances than I imagine can elsewhere be found.

Above Durham the river meanders through a valley resembling that of the Irwell opposite Kersal-moor, one or both of the opposite banks being composed of drift. At Durham itself a firm dam of clay has been thrown across the old valley, and the river has cut a narrow horseshoe-shaped ravine in the solid rock, thus isolating the block of stone on which the cathedral and castle stand. As soon as the stream emerges again from the rock into the pre-glacial valley, the bounding cliffs fall back, and the valley widens below as above the town. The narrow isthmus which joins the isolated block of rock is called Clay-path, and the boulder-clay in it is decidedly tough.

There are other instances of this clay-bar across the old river valley, and the isolation of a block of rock connected with the opposite side by a clay mound which has usually by this time weathered much lower than the isolated rock above which at first it must have risen. There is a very beautiful example of this (one amongst many) on the Esk, by Lealholm Station, near Whitby.

At Sunderland Bridge Station on the N.E.R. main line to the south of Durham is one of these diverted channels, half cut down. The river has begun a semi-circular ravine in the solid rock, but after it had dug down some 40 feet the clay barrier failed, and the ravine is left apparently purposeless. The Sunderland-bridge Railway Station stands in the hollow, and the railway forms the sagitta, whilst the viaduct across the present valley of the Wear beyond marks the site of the former clay barrier. Narrow, rocky-sided, and deserted river valleys will therefore be usually of post-glacial formations.

This Wash, or old valley of the Wear, proves that before the glacial period the north-eastern part of England was at

least some 300 feet higher than at present, for a rise of some 200 feet would push the sea much further off, and yet only bring the bottom of the Wash at Newcastle about to sea-level.

The depth of these old effaced river-beds gives us surely the best index of land elevation in old times, and several interesting conclusions follow from it.

Rock stretches, I believe, close to the surface of the ground all across the mouth of the Tees estuary. If then we raise the land to pre-glacial elevation, the Tees valley here should be sunk some 200 feet below the surface. The Tees therefore, we may assume, in pre-glacial times flowed southward into the Ouse, as the Wear flowed northward into the Tyne.

The York plain and the vale of Malton represent the filling in of valleys whose rock bottoms, in the case of the plain of York, may be 200 or 300 feet below the present surface.

The section of the ravine excavated by the Wear since the glacial period is at least, I judge, forty times less in area than that excavated before. How old, then, are our English rivers?

It is therefore, I think, a tolerably safe rule that all narrow rocky valleys are post-glacial,—*e.g.*, I have no doubt the Nidd at Knaresbro', the Derwent at Castle Howard, Swale at Richmond, Esk at Whitby harbour, are cases of post-glacial section. In several of these cases we can see clearly the old filled-up valleys. The Derwent apparently formerly flowed out by Gilling, on to Pilmoor, &c., &c.

It is stated that scratched flints occur at the top of boulder-clay a little to the north of Sunderland. If this be so it proves that at the close of the glacial period there were chalk beds to the east, off Sunderland, in continuation of the York wolds, at such a height as to send glaciers backwards when the great ice-sheet had so far lessened as to permit the free play of ice on minor slopes.

Do the clay dams across the pre-glacial Wear valley and other similar valleys mark epochs in the decline of the ice when the retiring glacier for a time advanced again and mounted up a huge mass before it? It is possible, of course, that they do not really differ from the rest of the denuded drift, but on the other hand they appear in some degree to correspond in Weardale and Eskdale, the to two cases I know best. Lealholm corresponds to Bishop Auckland: above these points in both valleys I have not noticed any great filling-up of the valleys. One can even fancy a correspondence between some of the clay dams in the one valley and in the other as regards relative position and completeness.

Mr. THOMAS CARNELLEY, B.Sc., exhibited and explained the action of Edison's Electric Pen.

Ordinary Meeting, March 7th, 1876.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. R. S. DALE, B.A., exhibited Specimens of Crystals of Sulphate of Lead found in Alum Residue, and said that "In the manufacture of pure red liquor (alumina acetate), lead sulphate occurs as a bye-product from the admixture of alum and lead acetate. This lead sulphate is filtered to a stiff paste. In a cask of this pulp the very curious crystals I exhibit were found embedded in the centre of the mass, and, so far as could be seen, unattached. These crystals resemble the axes of a crystal of the *regular system*, and are undoubtedly pseudomorphs from crystals of alum. Lead sulphate in the crystalline state belongs to the *Rhombic system*. In the same cask crystals of alum were found, which leaves little doubt that such was the origin of these extraordinary forms. Analysis showed them to consist of lead sulphate."

"On the Degree of Accuracy displayed by Druggists in the Dispensing of Physicians' Prescriptions in different towns throughout England and Scotland,"* by WILLIAM THOMSON, F.C.S.

The results obtained by Mr. Allen, the public analyst for Sheffield, a short time ago in reference to the inaccuracies displayed by druggists in making up prescriptions, led me to believe that it would be interesting to have the same prescription dispensed by different druggists, in different parts of England and Scotland, and by analysis to decide the *range* of inaccuracies, if any. By the aid of my friend, Dr. Sinclair, of Manchester, to whom I am indebted for much subsequent help, I was furnished with two ordinary prescriptions, the principal ingredients of which admitted of very accurate determination, as I shall afterwards show.

The prescriptions were as follows:—

℞. Potass Iodid......	ʒij	℞. Zinci Sulphat.	℈ij
Sp. Chlorof.......	ʒj	Aq. Pur.......	℥ij
Aq. ad	℥vj	M. Fiat Lotio.	
M. ℥ss ter die.			

The processes of analysis were so simple for both that it leaves little doubt as to the accuracy of the results. The specific gravity of each solution was first taken. 100 grains measure at 60° Fahr. were then placed in clean, accurately tared and marked platinum capsules, weighing from 180 to 200 grains each; the fluids were then carefully evaporated to dryness on a water bath, those containing the potassium iodide being afterwards heated in an air-bath at 220° Fahr. till they ceased to lose weight, whilst those containing the zinc sulphate were dried at 220° Fahr. and afterwards heated to dull redness to drive off the last molecule of water

* The facts contained in this paper were accepted by the Committee of the Pharmaceutical Society of Great Britain, to be read before them, and subsequently, on the day advertised by them for its reading, rejected by the Council.

of crystallization, and the anhydrous zinc sulphate calculated into the crystalline or hydrated zinc sulphate; these prescriptions, then, contained no ingredient which could interfere with the direct determination of the salt introduced. I give the dispensers, in this paper, the advantage of not estimating the actual proportion of the pure salt, but the total, of what had been added by them. The first prescription should have been made up to a total fluid measure of 6 ounces (2625 grains) which quantity should have contained 120 grains of potassium iodide. The second prescription should have been made by adding 40 grains of crystallized zinc sulphate to 2 ounces of water, which would make a total fluid measure of 893 grains, but as few gave either the exact measure of liquid, or weight of solid, I found it necessary to make three columns of figures, in the following tables, for each prescription; the first to show the amount of liquid measured out; the second to show the total amount of solid weighed out; and the third, as a comparison of the actual strength of the different fluids, which is made by calculating the amount of potassium iodide which would be contained in exactly 6 ounces (2625 grains measure) of the mixture, and the amount of zinc sulphate which would be contained in exactly 893 grains measure of the lotion, supplied by each druggist.

It will, of course, be clearly seen, that if the potassium iodide or zinc sulphate were damp, or in bad condition, although the weighings may have been made with absolute accuracy, the actual amount of the salts found on analysis would be less than that weighed; but this is equally a fault, because dispensers ought to have all their drugs in good condition. The following table will show the results of the analysis of eighty-one samples of the potassium iodide mixture, and the same number of the zinc sulphate lotion, one sample of the mixture, and one of the lotion, having

been dispensed by each druggist; besides which, at the suggestion of Dr. Sinclair, I have annexed the prices charged by each, for the two bottles, as in his opinion it might prove of general interest to dispensers, and will make the table more perfect, because from those who charge most, the greatest degree of accuracy should be expected. I may further state, that from each important town, I endeavoured as far as possible to have one lot dispensed by a druggist having the highest reputation and another by one of the lowest class, but I found it difficult to carry out this exactly, so that the prescriptions have been made up more generally by high class or respectable druggists than by those of a lower class. 1 have, however, as far as possible, marked those who could be recognised as having decidedly large and respectable shops, and those that were decidedly low class; the others may all be accepted, I believe, as respectable, and many may even be termed high class druggists.

TABLE I.

It might be well to mention here with regard to the verification of these figures, that the analysis of each sample which deviated beyond five grains in the potassium iodide, or zinc sulphate, from the prescribed amount, was repeated, and the result, of the second analyses found in each case to agree with that of the first. The specific gravities of all the lotions closely coincided with the amounts of zinc sulphate found, but in the mixtures, owing to the different amounts of spirit of chloroform which had been added, on the one hand, and the difference in the actual composition of that spirit of chloroform on the other, the specific gravity was no indication to the quantity of potassium iodide present. In looking over the above table it will be seen that only two druggists out of the eighty-one have given exactly the required weight of potassium iodide; thirty-four have given *more* than the prescribed amount, and forty-five *less;* but it may be of further interest to notice that when the whole of the quantities of potassium iodide given by the eighty-one different druggists are added together that the total quantity comes to 220½ grains *less* than it would have been if each druggist had dispensed the exact quantity. Again, in the lotion, only one druggist out of the eighty-one gave the exact weight of zinc sulphate; forty-three have given *more* than the prescribed amount, and thirty-seven *less*; and when the whole of the quantities of the zinc sulphate given by the eighty-one different druggists are added together it comes to only 12½ grains *more* than it would have been if each druggist had dispensed the exact quantity. This *résumé* seems to show that a larger percentage of druggists have given less weight for the more expensive drug, viz., potassium iodide, than for the zinc sulphate, the value of which is infinitesimally small, but still, no one can come to the conclusion that this is really done with dishonest intention in the large majority of cases.

I think, however, that no one can have a doubt about the want of care which is shown generally in dispensing, by the above table. A large percentage have dispensed within a range of accuracy which many might consider reasonable. I have, however, made all my estimations with analytical accuracy, and I think it must be left to the medical profession to decide what limits of error they consider might be allowed. With the view to decide what amount of inaccuracy a pharmacist would consider allowable, I consulted a gentleman who is a partner in an establishment which does a considerable business in dispensing. After informing him of the investigation I had been making, I asked him what amount of inaccuracy he would consider allowable in dispensing 120 grains of potassium iodide in 6 ounces of fluid, and also for 40 grains of zinc sulphate in 2 ounces of fluid; he considered that in both cases they ought to be absolutely accurate, but if I allowed three-tenths of a grain either way I should be allowing sufficient for all practical purposes. I have, however, been still more lenient than my pharmaceutical friend, and have allowed five-tenths of a grain on either side of the prescribed quantity as the range of practical accuracy. I know that many dispensers will take objection to this range of inaccuracy as impracticable. We, as analysts, can weigh easily to the one-hundredth part of a grain, and I know that balances used by dispensers for weighing such quantities as 120 grains are capable of turning with the tenth part of a grain if kept in good condition, and I think under such circumstances it would be absurd for any one to contend that it is impracticable to weigh drugs within half a grain on these premises. I have formed the following summaries of the above results:—For the potassium iodide mixture, two druggists out of the eighty-one have given the exact weight prescribed; nine out of the eighty-one have come within the practical

range ot accuracy; fifty-five out of the eighty-one have weighed within 5 grains either way of the prescribed amount; whilst the remaining twenty-six have made greater errors. For the zinc sulphate lotion, one druggist out of the eighty-one gave the exact weight prescribed; nineteen out of the eighty-one have come within the practical range of accuracy; fifty-one out of the eighty-one have weighed within 2 grains either way of the prescribed amount; whilst the remaining thirty have made greater errors.

In the actual measuring of the fluids, I have assumed that measurements within 5 fluid grains either way are absolutely correct, whilst those within 15 grains either way are practically correct.

For the potassium iodide mixture, six dispensers out of the eighty-one have measured correctly; eleven out of the eighty-one have come within the range of practical accuracy; thirty-two have measured within 50 grains (a teaspoonful) of the prescribed amount; whilst the remaining forty-nine have made greater inaccuracies. For the zinc sulphate lotion, six dispensers out of the eighty-one have measured correctly; sixteen have measured within the range of practical accuracy; twenty-eight have measured within 25 grains of the prescribed amount; whilst the remaining fifty-three have made greater inaccuracies. Lastly, with respect to the strength of the solution, some dispensers may make both their weighings and measurements in excess or deficiency, and in either case the strength might be exactly what is required; whilst others may have weighed correctly and measured incorrectly, or *vice versâ*, and in these instances, the strength of the solution, which is the most important point, would be wrong. The following shows the amount ot deviation made in this respect :

Not one dispenser has succeeded in making the prescription to the exact strength in either the mixture or lotion.

In the potassium iodide mixture, five out of the eighty-one dispensers have come within the range of ½ a grain more or less than the prescribed amount; forty have made the strength of the mixture within 5 grains more or less than the prescribed amount; whilst the remaining forty-one have made greater errors.

In the zinc sulphate lotion fourteen out of the eighty-one dispensers have come within the range of ½ a grain more or less than the prescribed amount; forty-five have made the strength of the lotion within 2 grains more or less than the prescribed amount; whilst the remaining thirty-six have made greater errors.

It may be interesting, before leaving this part of the subject, to make a few further observations on the dispensing of these solutions. We found that the mixture of No. 74, dispensed by a man in Birmingham, was strongly alkaline to test paper, and I submitted its contents to further analysis and found, that out of the 115·7 grains represented in the table, 100·1 was composed of carbonate of potash, and 15·6 of iodide of potassium, etc. From this large proportion, it seems as if the former salt had been intentionally added, along with a small proportion of potassium iodide. One (No. 48) from Eccles contained 2·5 grains of Potassium Carbonate in the 126·7 grains weighed out. Many were absolutely free from Potassium Carbonate and many contained traces of that salt. No. 46 had both the mixture and lotion corked with very dirty corks. The dispenser of No. 16 (from Edinburgh) put in a preparation of orange

instead of spirit of chloroform. No. 4 (from Cupar Fife) added the spirit in such proportion that it possessed the smell of whisky; whilst No. 18 (from Airdrie) dispensed the chloroform without any spirit, so that it remained insoluble at the bottom of the bottle. This error might have proved serious if the last dose in the bottle, containing all the chloroform, had been swallowed by the patient. The seven mixtures to which the following numbers relate contained disagreeable looking sediments—17, 24, 45, 46, 56, 74, and 78. One more potassium iodide prescription was made to contain the same quantity of salt as the others, but the solution made up to two instead of six ounces. The following shows the result:—

TABLE II.

POTASSIUM IODIDE PRESCRIPTION.

No.	District.	Description of shop	*Actual measure* of the mixture dispensed. (In fluid grains.)	*Actual amount* of Potassium Iodide weighed out by the Druggist.	*Strength* of the mixture calculated on the two ounces.	Price.
The mixture as prescribed.			875	120 grs.	120 grs.	*s. d.*
1	Manchester: Stretford-road		895	123·1	120·4	1 2

With the view to test further the range of inaccuracies in other and more valuable medicines, Dr. Sinclair and I arranged to have a few different prescriptions dispensed, and he accordingly wrote out five, having the following composition:—

℞. Argent. Nitrat. .... ʒ j
Aq. Distillat........................... ℥ j

M. Fiat lotio. To be kept from the light.

These were subject to analysis, and the following Table shows the results:—

Table III.

Silver Nitrate Prescription.

No.	District.	Description of shop.	*Actual measure* of the lotion dispensed. (In fluid grains.)	*Actual amount* of Silver Nitrate weighed out by the Druggist.	*Strength* of the lotion calculated on 447·5 gr.	Price.	
The lotion as prescribed.			447·5	60·0	60·0	*s.*	*d.*
	Manchester:						
1	Moss Lane W.		410	59·8	65·3	1	0
2	do. London-rd.	Low.	425	44·8	47·2	1	0
3	do. Oxford-st...		425	57·4	60·4	1	6
	Liverpool:						
4	Gt. Homer-st..	Low.	433	73·2	75·6	1	4
5	London, E.C....		365	59·0	72·3	0	8

The figures in this table show the amounts of anhydrous silver nitrate contained in the solution.

These show that not one of them has given the weight of this drug accurately; one came within the range of practical accuracy; three came within the range of 5 grains, and two made inaccuracies of upwards of 13 grains. In measuring, none came within the range of absolute accuracy, viz., 5 grains either way, and only one came within the range of practical accuracy. In strength, one came within the range of practical accuracy, the others made errors of over 5 grains.

The next prescription was the following :—

℞. Quin. Sulphat. .... ʒ j
Acid. Hydrochlor. dil............ ʒ j
Aq...............................ad. ℥ ij

M. Sig. One teaspoonful to be taken in a wineglass of water twice a day.

Two of these prescriptions were dispensed, and three more containing the same amounts of quinine sulphate and hydrochloric acid, but made up to 6 instead of 2 ounces measure.

These were submitted to analysis, with the following results:—

TABLE IV.

QUININE SULPHATE PRESCRIPTIONS.

No.	District.	Description of shop.	*Actual measure* of the mixture dispensed. (In fluid grains.)	*Actual amount* of Quinine Sulphate weighed out by the Druggist.	*Strength* of the mixture calculated on the two ounces.	Price.
The mixture as prescribed.			875	60 grs.	60 grs.	*s. d.*
1	Liverpool: Lime Street		920	59·7	56·8	3 6
2	London, E.C.	Low.	900	42·0	40·6	2 6

In No. 2, the hydrochloric acid of the prescription had not been introduced, and most of the quinine sulphate remained undissolved.

No.	District.	Description of shop.	*Actual measure* of the mixture dispensed. (In fluid grains.)	*Actual amount* of Quinine Sulphate weighed out by the Druggist.	*Strength* of the mixture calculated on the six ounces.	Price.
The mixture as prescribed.			2625	60 grs.	60 grs.	*s. d.*
3	L'ncaster, Town		2660	56·8	56·1	3 0
4	Manch. Lnd.-rd.		2700	64·5	62·7	1 6
5	Liverp'l, Bootle		2810	59·7	55·8	2 2

The figures in these two tables represent the amounts of Quinine Sulphate containing 7 molecules of water of crystallization.

In this it will be noticed that in the quantities weighed none of the five dispensers arrived at absolute accuracy, two came within the range of practical accuracy, and the remaining three are outside this mark; none measured within the range of either absolute or practical accuracy, and none came within the range of either absolute or practical accuracy in the strength of their solution.

The third and last prescription was the following:—

℞. Ferri et Quin. Citrat............ ʒij
Aq................................ ℥vj

Sig. ℥ss, ter die.

Two of these prescriptions were dispensed, and one containing the same amount of salt, but made up to 2 instead of 6 ounces.

The results of the analysis are as follows :—

TABLE V.

IRON AND QUININE CITRATE PRESCRIPTIONS.

No	District.	Description of shop.	*Actual measure* of the mixture dispensed (In fluid grains.)	*Actual amount* of Quinine and Iron Citrate weighed out by Druggist.	*Strength* of the mixture calculated on the 2690 grains.	Price.
The mixture as prescribed.			2690	120 grs.	120 grs.	*s. d.*
1	Manchester: Hulme.		2690	122	122	1 6
2	London, E C.		2570	140	146·5	1 9

No.	District.	Description of shop.	*Actual measure* of the mixture dispensed (In fluid grains.)	*Actual amount* of Quinine and Iron Citrate weighed out by Druggist.	*Strength* of the mixture calculated on the 940 grains.	Price.
The mixture as prescribed.			940	120	120	*s. d.*
3	Manchester: Oxford.road.		985	107	102·1	2 6

The figures in these tables represent the dry iron and quinine citrate, plus 10·5 per cent, the amount which we found the salt to lose on drying at 212° F.

Not one of these three came within the range of absolute ·r practical accuracy in either the weight or the strength of ·ution. One, however, measured with absolute accuracy,

the remaining two were out of the range of practical accuracy in every respect.

In concluding, it may be of some importance to mention that in the dispensing of these prescriptions, in the large majority of cases, and generally in the more respectable shops, no questions were asked of the purchasers, and no remarks made, but in some cases, and especially in those shops of a lower class, questions of rather an impertinent nature were asked ; in one, not only was the patient's name demanded, but the name of the medical man who prescribed; and in another instance the druggist actually refused to dispense a prescription containing 10 grains doses of quinine sulphate on the ground that the dose was excessive, and one who did dispense it remarked that the dose was a strong one. The bearing of these facts on the relative position of the physician, patient, and druggists, although of much importance, especially to the medical profession, does not come within the scope of my paper.

In conclusion I must express my best thanks to our assistant, Mr. Percy J. Winser, for the painstaking and accurate manner in which he had helped me in this investigation

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MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 14th, 1876.

CHARLES BAILEY, Esq., in the Chair.

Mr. E. W. Nix, M.A., was elected a Member, and Dr. John Roberts an Associate of the Section.

Mr. Percival, through Mr. Rogers, exhibited specimens of a new British moss—Hypnum nitidulum (Wahl), belonging to the sub-genus Plagiothecium—found by him and Mr. Whitehead on June 8th, 1868, at Penneghant Gill, Craven,

Yorkshire. It differs from its near ally, H. pulchellum (Dicks), in its leaves being broader and larger, and in the male flowers being separate and distinct. Besides, H. nitidulum affects rotten wood, decayed bark, or decomposed vegetable matters as a site for its growth; whilst H. pulchellum always grows in the fissures of rocks.

Mr. Plant exhibited a rare hydrocarbon mineral from peat found near Manchester, The mineral crystals which encrust and radiate over the inner surface of the pine bark in the specimen exhibited, belong to the Fichtelite group of the hydrocarbons, and may be subsequently determinable to this. If so, it will be the first instance of Fichtelite having been recorded as occurring in Great Britain.

Mr. Plant then read a paper on the gradual decrease of wild birds during the last 25 years west of Manchester. The list included all the birds—residents, visitants, and casual stragglers—observed in and near to Peel Park, in the years 1850-60-70, and the numbers resident in 1871-75.

The birds recorded in 1850 were 71; 1860, 42; 1870, 19; and in the last five years only 8 resident birds were to be found.

The causes for the decrease were chiefly increase of building, destruction of trees from many causes, and general absence of food and shelter for birds.

Ordinary Meeting, March 21st, 1876.

EDWARD SCHUNCK, PH.D., F.R.S., &c., President, in the Chair.

Dr. Arthur Schuster exhibited an interesting collection of objects brought by him from Siam and the Western Himalayas.

"On a Graphical Method of Drawing Spectra," by Mr. WILLIAM DODGSON, Whitworth Scholar. Communicated by Professor ROSCOE, F.R.S., &c.

Construction of Curve for ascertaining the wave-lengths.

A number of points are first plotted on the curve-paper, the abscissæ of these points being the micrometer scale-readings of certain lines, as observed directly in the spectroscope, or being absolute—or relative measurements taken from an existing photograph; whilst the ordinates of these points are the corresponding wave-lengths, taken from the determinations of some reliable observer.

If there be a large number of points taken, the curve ought not to be drawn actually through them all, or it would probably be irregular; it therefore becomes necessary to draw a mean curve, *i.e.*, a continuous even curve which shall pass near to all the given points, some being on one side and some on the other.

The following method was employed to determine the wave-lengths of the well defined edges of the bands in two absorption spectra.

The micrometer scale readings of 27 air-lines were observed in the spectroscope, in addition to those of the absorbing vapour, and taken as abscissa; and the corresponding wave-lengths determined by Thalén as ordinates. These 27 points when plotted on the curve-paper, fell naturally into six groups; a mean point was then found for each group by accurate measurement, and the curve drawn through these

six mean points, such that the sum of the shortest distances of the several points of any one group from the curve is zero.

The mean points were obtained by means of the arrangement shown in Fig. 1.

The wooden rod AA has a steel point CC fixed at one end, and a graduated scale of tenths of an inch at the other; the axis of the steel point is kept vertical and the scale horizontal while the rod is being rotated, by the strip of wood TTP, the end P sliding on the table. The centre C being placed at some fixed point, the curve-paper is then adjusted on the table, so that some particular division on the scale (found by trial) describes a circle through a group of points, and also through two adjacent groups, one on each side, which circle if drawn would pass not far from the mean points of the three groups we are proceeding to determine. The distance from each point of the middle group to the centre C is then read off on the scale to the $\frac{1}{1000}$th of an an inch, with the aid of a lens, and a second scale divided into $\frac{1}{100}$ths of an inch; and a point plotted near the centre of gravity of the middle group at a distance from C equal to the mean of these readings.

This process is repeated for each of the four middle groups, the mean points of an end group being determined with the paper and centre C in the same positions as for the second group from that end.

A nearer approximation to the true position of the mean points may then be found, by making use of those already obtained, to find more correctly the radius of the circle through the three groups, and repeating the process of measuring and plotting.

In one case an isolated point occurs; this is taken as one group, and the curve drawn through it.

The curve consists of arcs of circles, which pass through the six mean points, with radii varying from 64¼ inches at the upper end, to 61½ inches at the lower end, drawn by means of the rod AA, having a drawing pen fixed to it.

The wave-lengths corresponding to the scale-readings of the observed lines in the two spectra, were then found by measurement from the curve to $\frac{1}{1000}$th of an inch, by means of a lens, and scale of $\frac{1}{100}$ths of an inch.

The above method of graphically drawing a curve, may be extended to the case where the points are nearly uniformly distributed, and not divided into groups naturally as in the case of the air-lines.

Divide the points into any number of groups, about an equal number in each group. Find the mean points as described, by aid of lens and vernier scale on the rod; draw the curve through the points by the best practical method. The number of groups taken will be guided by judgment, and will depend on what degree the proposed curve shall be. The greater the number of groups, the greater will be the degree of the equation to a curve passing through their mean points, and the greater probably will be the irregularities in the variation of the radius of curvature.

Construction of Scale for Map of Spectrum.

The arrangement shown in Fig. 2 has been employed to draw the map scale, and found to be an accurate and expeditious method.

ABCD the drawing board, EFGH the drawing paper, TT Stanley's ebony-edged T-square, resting against the pins RR during the operation; MM Stanley's ebony-edged set-square, which slides along the edge of TT; SS Stanley's engine-divided, millimetre scale, connected at one end to the edge of the set-square M, by pins and elastic string NN, so as to slide with the set-square, and have its graduated edge parallel to the edge of TT, and also parallel to the lines KK, K′K′ on which the scales have to be constructed; LL a very fine ink line at right angles to the edge of the scale, beyond the extremity of the required scale KK, on the right hand side, to which the divisions of the millimetre scale are adjusted by the eye, with the aid of a lens placed on a stand, its axis in a vertical plane through the line LL; a fine adjustment of the line and division is obtained by means of the blunt handle of the drawing pin, used as a lever against the end of the scale, the surface of the board or the paper being the fulcrum.

The divisions of the map scale are drawn at Z,Z, with a very thin drawing pen, very close to the edge of the set square, so that the variation of the inclination of the drawing pen is a very inappreciable error. If the map scale is required to be longer than the scale S, another very fine perpendicular ink line L′L′ must be drawn at an exact multiple of 10 millimetres distant.

To prevent mistakes in drawing the principal divisions, the line LL is arranged so that they may be drawn when the corresponding principal divisions on the scale SS are coincident with the line.

The advantages of this method of drawing a map scale, arise from the fact that the error due to the observation of the coincidence of two lines, is much less than that of plotting a point opposite each division of the millimetre scale. Also in the fact that the error due to drawing a line with a thin pen very close to the edge of the set-square,

(the pen approximately at the same inclination) is much less than that of drawing the divisions through the plotted points, judging by the eye.

If only one scale be required on the map, place the edge of the scale S near to the line KK. If two are required, place the edge of SS half-way between KK, K′K′ to lessen the errors due to imperfections in the straight edge of the T-square.

Construction of a Vernier Scale for the above Map Scale.

The drawing board, T-square, set-square, and paper as in Fig. 2. OO a strip of paper, on which the vernier scale is to be constructed, its edge being parallel to TT, the scale SS is attached to the edge of the set-square and slides with it, by means of the pins and elastic NN as before, but inclined to the edge of TT at an angle $\alpha = \sec.^{-1} \frac{37}{36}$; it is kept in this position by the pegs U, U in the end of the scale; the fine ink line LL is drawn at right angles to SS, the lens being used on a stand as before, with its axis always in a vertical plane through LL.

The divisions on the vernier scale will be $1\frac{1}{36}$ millimetres in length.

"Evidence to prove that a Bone from the Windy Knoll, Castleton, named by Professor W. Boyd Dawkins, F.R.S.,

'Sacrum of young Bison,' is a Sacral Bone of the Cave Bear, *Ursus Spelæus*," by JOHN PLANT, F.G.S.

When I first described, at the Manchester Geological Society in April, 1874, a number of bones which came from a limestone fissure near Castleton, they were correctly attributed, even upon a cursory examination, to be the remains of Bison, Reindeer, Wolf, and Bear, the bulk of them belonging to the Bison. In the course of a careful examination subsequently, several of the bones were found, presenting characters not to be determined by the aid only of such scanty help for comparison which was at my service, so I took a number of them to the Liverpool Museum, and, with the kind assistance of Mr. Moore, the Curator, was able to compare them with skeletons of bovine and ursine animals, as well as with a collection of bones of the Cave Bear —*U. Spelæus*—which the Museum possessed, from La Grotte des Echelles, France. Some of my specimens could not be determined then, and have not yet been identified; but in one case both Mr. Moore and myself were quite of the opinion that it bore a close resemblance to a sacral bone of the Cave Bear, in their collection from La Grotte des Echelles, and I ventured without hesitation to ascribe it to that animal.

At a meeting of the Geological Society in May, 1874, I specially exhibited this bone, and described the new interest which had become attached to it. Professor Dawkins, however, expressed his doubts as to the correctness of my naming, and I at once placed the bone in his hands so that he could satisfy himself upon the matter. A month after (June 24) I received the bone, with a short note saying;— "*The bone which I have ordered to be returned is sacrum of young Bison,*" and upon the bone itself was written, "*Bison, W.B.D.*"

Surprised, but not convinced, I paid another visit to the Liverpool Museum, and with my friend Mr. Moore recom-

pared this identical bone with the sacral bones of all the bovine skeletons in the Museum, and from merely having an opinion, I became positively certain of my first determination of the bone, and equally certain that my specimen did not, and could not, belong to a bovine animal.

In the Proceedings of the Literary and Philosophical Society of Manchester, October 6th, 1874, p. 6, Professor Dawkins states—"The Cave Bear, or *Ursus Spelæus,* is also stated by Mr. Plant (Manchester Geological Transactions, xiii, 130—156) to have been discovered in the Windy Knoll fissure, principally on the *fancied resemblance* which a sacrum of a young animal bore to a sacrum in the Peel Park Museum, *said to belong to Ursus Spelæus,* partly also on the stumps of two teeth, worthless for purposes of specific identification. *I have carefully analysed this evidence, and on comparing the sacrum in question with that of the ox and bear, I believe that it belongs to a young bison, and not to any carnivora.* And, further, even if it belong to a bear, there is no evidence as to the species, because the specific characters of that bone in the fossil bears have not yet been ascertained. The researches of Professor Busk, during a long series of years, and my examination of the most important collections of fossil bears in this country and in France, prove that the determination of the species is a point of extreme difficulty, and we are only able to detect characters of specific value in the heads and dentition. On this point I would refer to Professor Busk's memoir, and to the vast collection at Toulouse. The Cave Bear, therefore, of Windy Knoll must be given up, as being based on a faulty determination."

It was a great surprise to me to find Professor Dawkins deliberately stating "that he had compared my specimens with the sacral bones of the ox and bear, and that it belonged to a young bison and not to any carnivora." I am aware that he is credited with the possession of great

experience and knowledge in the bones of Pleistocene animals, and must know the risks of damaging that reputation in giving an opinion upon a matter like this without full consideration; but as he had the bone in his possession for nearly one month, it could not be said he formed his opinion in a hasty manner.

Notwithstanding this deliberately expressed opinion of Professor Dawkins that it belonged to a young bison, I was not alarmed for my small reputation as a palæontologist, or convinced that he was right. I had myself seen and examined sacral bones of both oxen and bears, and always found the differences between them far too striking to be mistaken. I was sure this bone could never be bovine, and I had a firm opinion that it was Cave Bear; moreover, I considered that nature could never be so hard up for variety as to give to the stiff-backed oxen tribe a sacrum that could be confounded with the sacrum of the lithe and flexible hindquarters of the bears.

However, I set myself to the study of bears and oxen, took the bone with me whenever I had a chance of comparing it with bones in public museums and private collections, and sent it for examination and report to gentlemen, who have made comparative osteology their special study, and without exception the opinions I received were unanimous in one respect, that it was not the sacrum of a bison, or even of a bovine animal—but the *sacrum of a bear*.

It is not necessary, nor do I intend, to mention the names of gentlemen to whom I here allude; their opinions were formed pretty much like my own by direct comparison only, without going into the minute anatomical points of the differences of structure between sacral bones of bovines and ursines. Comparison is usually a safe method of determining species in natural history, and it applies with equal force with bones, or portions of bones, when authenticated specimens can be obtained for comparison.

Being in London in February, I waited upon Professor Owen, at the British Museum, and submitted the bone to his experienced judgment. He most kindly undertook to make a thorough examination of the bone, and give me his opinion. A few days after I called upon him again, when he took great pains to show me a number of sacral bones of bears and bison, and point out the clear differences between them in certain points, and in what way it could be proved that my specimen belonged to the bear and not to the bison, at the same time giving me the results of his determination in writing, with full permission to use it in any way that would serve the purpose of my enquiry.

"British Museum, *8th February*, 1876.

"The fore end of the sacrum yields the best and readiest characters for distinguishing that of an ox from that of a bear. In the *Ruminant* the pre-zygapophyses present each a long longitudinal semi-cylindric channel, for the corresponding convexity of the post-zygapophyses of the last lumbar vertebra.

"In the *Bear* the pre-zygapophyses of the first sacral, present a short suboval flat, or almost flat, surface, which becomes slightly concave below through the production of the mesial border of the process.

"In the Ox the symphysial surface for the ilium is in great part presented by the transverse process of the first sacral, which is much produced, these processes give a winged character to the fore part of the sacrum; the surface itself is long, narrow, and looks backward more than outward.

"In the Bear the transverse process of the first, is little longer than of the second, sacral, and it is much thicker in proportion to its length than in the Ox. The second sacral contributes a larger proportion to that surface than in the Ox.

"The neural spine of the first sacral in the Bovines is long (lofty), compressed, and becomes confluent with the second, as this with the succeeding sacral spines, to form a continuous bony crest.

"In Ursines the sacro-neural spine in the first vertebra is small and short; in the second often almost obsolete, that of the third sacral, though small and short, is usually better developed, but all remain distinct from each other, with well marked intervals, except along their base.

"No palæontologist who had ever made this comparison could risk the mistaking of a bear's sacrum for one of a bovine quadruped.

"RICHARD OWEN."

Before leaving Professor Owen I pointed out to him that the bone he had taken so much trouble to compare with the large series of specimens before him was marked "Bison, W.B.D.," and had been so named by Professor Dawkins, and I also informed him that it had previously been submitted to the care of Mr. Wm. Davies, acknowledged to be the most experienced comparative osteologist in the Zoological Department in the British Museum, and also to the assistant in this department, Mr. Gerrard (who has charge of the skeletons), who, although at first inclined to the opinion that the bone had the aspect of a bovine sacrum, after careful examination, agreed with his colleague Mr. Davies in his determination, which is this—"Undoubtedly the first and second sacral vertebræ of the Cave Bear, *Ursus Spelæus*.—WM. DAVIES."

The last remark of Professor Owen was that this opinion from Mr. Wm. Davies was quite sufficient in itself to have settled the question of the species of the bone. His own remarks had been solely directed to explain the marked differences between the sacral bones of oxen and bears.

The drawings I exhibit to assist in showing the special points alluded to by Professor Owen, the specimens of sacral vertebræ of oxen, bison, and the sacrum of Cave Bear from the Liverpool Museum, to compare with the sacrum from Castleton, will, I doubt not, be convincing, and prove beyond all doubt that the bone in question was at the first correctly lescribed when I named it sacrum of *Ursus Spelæus*.

PHYSICAL AND MATHEMATICAL SECTION.

October 12th, 1875.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the chair.

"On a Source of Atmospheric Ozone," by JOSEPH BAXENDELL, F.R.A.S.

The source of atmospheric ozone is a subject respecting which various opinions have been entertained by chemists and meteorologists. According to M. Schönbein, Mr. Dancer, M. Houzeau, Professor Roscoe, and others, it is due to electrical action. Dr. Daubeny, from a long series of experiments, was led to attribute it to the action of sunlight on the green leaves of plants. M. Gorup-Besanez believes it is simply the result of evaporation; and Mr. Mackereth regards it as being dependent on a peculiar and direct action of the sun's light upon the oxygen of the atmosphere.

In my paper "On Observations of Atmospheric Ozone," read October 20th, 1868, I stated that from my own observations it seemed probable that the amount of ozone near the earth's surface depended upon the height at which clouds are formed in the atmosphere. Subsequent observations confirmed this view, and also showed that the amount of ozone had, in general, some relation to the degree of transparency of the lower atmosphere. Thus when fogs and haze were prevalent it rarely happened that even the faintest trace of the presence of ozone could be obtained; but on their disappearance the indications became more marked, and in clear states of the air the test papers were generally more or less coloured. It appeared, therefore, from these results that atmospheric ozone was absorbed or

decomposed by haze or fog, and was given off or produced when evaporation changed haze or fog into invisible aqueous vapour.

From the very small degree of solubility of ozone in water it seems highly improbable that the amount of water existing in ordinary haze or fog could absorb the whole of the ozone usually found in the atmosphere, nor could the whole of this ozone be contained in the water from which the aqueous vapour in the air had been derived. Moreover, not only does haze prevent the coloration of the test papers, but it often rapidly bleaches those which had already been coloured by the action of ozone. This bleaching effect, however, is not produced if the papers are thoroughly wetted by immersion in water, and therefore it has been attributed to the action of a form of oxygen differing essentially in some of its properties from ozone, and which has, therefore, received the name of antozone.

The existence of antozone is, however, doubted by many chemists, and it becomes, therefore, necessary to seek some other explanation of the bleaching effect of fogs and haze, which will also, at the same time, account for their effect in apparently absorbing or decomposing ozone, and afterwards of giving it off or forming it on evaporation.

As it is generally supposed that the minute vesicles or globules of water which form fog and haze are similar in all respects, except perhaps in size, to the globules which form the spray from a breaking wave, a waterfall, or fountain, it was evidently desirable to ascertain whether the spray from any of these sources would produce effects similar to those produced by fog and haze, since it might be contended that as in the one case the globules were the result of a condensation of aqueous vapour taking place in the air, while in the other they were produced by a more or less violent mechanical action, their effects upon the oxygen or ozone in the atmosphere might be very different.

In 1872 I had a series of observations made twice daily for five weeks at stations on opposite sides of one of the reservoirs of the Manchester Corporation Waterworks, and, grouping the results according to the direction of the wind, I found that the mean values for the station on the lee side of the reservoir were not sensibly different from those at the station on the windward side. The surface of the water was, however, seldom sufficiently disturbed to produce any appreciable amount of spray; but the results of the observations clearly indicated that mere evaporation from a large continuous surface of water had no effect in increasing the amount of ozone in the air passing over it.

Observations and experiments made at Southport on the action of spray from the sea led to no satisfactory result on account of the difficulty of eliminating the effects due to varying velocities of the wind; but the influence of fogs and haze in checking the coloration of the test papers was very marked.

I had long been anxious to try the effects of spray from a large fountain, but had no opportunity of doing so until June last, when I became aware for the first time of the existence of the fine fountains at the Arnfield and Hollingworth reservoirs in the Manchester Corporation Waterworks' district. In both these fountains the diameter of the column of water as it issues from the discharge pipe is nine inches, and under full pressure the height of the column or jet is about 27 feet at Arnfield, and 35 feet at Hollingworth. The water is turned on at very irregular intervals, and generally for only short periods of time, but with the kind assistance of Mr. James Wilkins, the very intelligent reservoir-keeper, and observer at the Arnfield meteorological station, I succeeded in making a satisfactory series of experiments, the details of which are as follows:—

For the exposure of the test papers two open boxes were used, each about 2 feet long, 10 inches wide, and 4 inches

deep. These boxes were placed on end, one on the windward, the other on the leeward side of the fountain, and the test papers were pinned in the upper ends of the boxes, and the boxes so placed that the papers were not exposed to direct sunlight, nor to wind and rain.

The first experiments were made on June 16, 1875, and three of Schönbein's test papers were placed in each box and exposed from 9.30 a.m. to 1.30 p.m. At the Arnfield fountain the resulting tints were:—

In Windward. Box (W).	In Leeward. Box (L).
4·5	7·0
5·5	7·0
5·2	8·0
Mean = 5·07	Mean = 7·33

At the Hollingworth fountain they were:—

W.	L.
5·0	6·5
4·0	6·5
5·0	7·5
Mean = 4·67	Mean = 6·83

Fresh papers were exposed from 1.30 to 6.30 p.m. with the following results:—

ARNFIELD.

W.	L.
5·5	8·0
7·5	7·0
6·0	5·0
Mean = 6·33	6·67

The third paper in the L. box was evidently a faulty one.

HOLLINGWORTH.

W.	L.
5·0	6·5
4·0	6·5
4·0	6·0
Mean = 4·33	Mean = 6·33

And the mean results of the two experiments were:—

	W.	L.
Arnfield	5·7	7·0
Hollingworth ...	4·5	6·6

July 2 and 9.

Experiments were made on both these days, but the atmosphere was hazy and rain fell, and none of the papers were coloured.

July 10—ARNFIELD.

Three papers, exposed in each box from 2.20 to 7.20 p.m.; but at 4.45 it was noticed that the wind had changed and the boxes were removed into fresh positions. The height of the jet was only 11 feet. The tints were:—

W.	L.
5·0	4·2
4·5	6·0
4·5	5·5
Mean = 4·66	Mean = 5·23

July 11—ARNFIELD.

Three papers in each box exposed from 9.30 to 11.40 a.m.; height of jet about 16 feet; very cloudy, but the air pretty clear.

W.	L.
4·0	5·5
3·5	5·0
4·0	4·0+
3·83	4·83

At 11.50 a.m. one paper was left in each box, and at 4.50 p.m. the tints were—

W.	L.
3·5	5·0

The weather was showery, and the wind oscillated between W.S.W. and W.N.W.

At 7.0 p.m. two papers were left in each box, and at 8.0 the following morning one paper was taken from each box, and the shades of colour were found to be—

W.	L.
2·0	3·0

At nine a.m. the remaining papers were both No. 4.

July 15—ARNFIELD.

Three papers exposed in each box from 2.30 to 4.40 p.m.; height of column of water about 20 feet; wind blowing in strong breezes from E.N.E.; air pretty clear, but sky very cloudy. The results were :—

W.	L.
3·5	4·0
3·0	3·5
2·5	3·5
Mean = 3·00	Mean = 3·66

Fresh papers were exposed from 4.50 to 7.5 p.m., with the following results :—

W.	L.
4·0	4·0
3·5	4·5
3·5	5·5
Mean = 3·66	Mean = 4·66

At 9 p.m. two papers were left in each box, and at 5.0 the following morning the results were—

W.	L.
7·5	9·0
8·0	9·0
Mean = 7·75	Mean = 9·0

August 11—ARNFIELD.

Two papers in each box from 12.30 to 3.30 p.m.; height of jet about 16 feet; temperature of the air at 12.30, 70°.5; temperature of evaporation, 63°.0; wind, S.W. to W.S.W. in light breezes.

W.	L.
2·5	4·5
3·5	5·5
Mean = 3·00	Mean = 5·0

One paper in each box was afterwards exposed from 4.0 to 7.0 p.m., when it was observed that the air was becoming hazy.

W.	L.
2.0	3.0

August 12.

Two papers in each box from 12.0 to 4.0 p.m.; but the air was very hazy, and there was no action on any of the papers. A thunderstorm occurred between 4.0 and 6.0 p.m., and at 7.0 p.m. the papers were again examined, with the following results :—

W.	L.
0·0	1·0
0·0	2·5
Mean = 0·0	Mean = 1·75

August 13—ARNFIELD.

Two papers in each box from 1 to 2.30 p.m.; air pretty clear; light breeze from W.S.W.; jet 16 feet. The spray box L was 9 yards from the jet. The distance on former occasions had varied from 7 to 17 yards according to the strength of the wind.

W.	L.
1·0 (A)	3·0 (A)
3·5	5·0

The papers marked (A) had been in my pocket-book since the previous day, but the others were fresh from the ozone box. A thunderstorm occurred between 4 and 5 p.m.

August 14.

At 10 a.m. two papers were placed in each box; at 12 no action had taken place on any of them, the air being very hazy. At 2.15 p.m. the air had become much less hazy and the tints were—

W.	L.
0·0	2·0
0·0	2·0

The height of the jet of water on this occasion was 21 feet. At 2.15 p.m. the readings of the dry and wet bulb thermometers were :—Dry, 68°·5 ; Wet, 61°.7.

The experiments thus briefly described appear to me to prove that the spray from a fountain on evaporating gives off or produces atmospheric ozone, and in this respect is similar to ordinary fog or haze.

In some of the experiments which I made the papers in the leeward box became sensibly damp in consequence of the box being placed too near the fountain, or from the acting of strong gusts and eddies of wind. In these cases the coloration of the papers was very much retarded or altogether prevented, and it therefore appears that spray has, like fog or haze, the power of absorbing or decomposing atmospheric ozone.

Having now proved, experimentally, that spray produced by mechanical means, and ordinary fog or haze produced by the condensation of aqueous vapour in the air, are precisely similar in their relations to atmospheric ozone; and having also shown that the quantity of ozone usually found in the air could not have been held in solution by the water from which the aqueous vapour in the atmosphere is derived it becomes evident that the production of atmospheric ozone is in some way dependent upon the minute state of division in which water exists in the air in the visible form of clouds, fogs, and haze, and often also probably in an invisible form. This consideration has led me to infer that water exposed to the air has the power of condensing Oxygen upon its surface into a thin film of ozone. When, therefore, complete evaporation of the vesicles or globules of moisture which constitute a cloud or fog takes place the ozone is left free to diffuse itself through the air; but when evaporation takes place from the surface of a large and practically inexhaustible mass of water the ozone is not set free but remains adhering to the surface It will, however, be objected that if ozone is formed in this way test papers ought to be coloured very rapidly in a fog or dense haze, and that no bleaching action could take place upon papers which had already heen ozonised. The explanation which I will venture to offer is that ozone associated with moisture and in the presence of the oxydised potassium will combine with the freed iodine and form iodic acid which, uniting

with the potash, will form the colourless iodate of potash.

But it may also be that the direct action of ozone on iodide of potassium is retarded or altogether prevented by an excess of moisture when the ozone is present in only small quantities as is usually the case in the atmosphere, and this view is supported by the fact that dry papers appear to be generally more sensitive than damp ones.

Admitting that the inference I have drawn from the facts given in this paper is correct, it will enable us to explain why, on the sea coast, winds from the sea bring more ozone than those from the land, and why, when the cloud stratum is high, the quantity of free ozone near the surface of the earth is, in general, greater than when the cloud level is low. It also indicates that the sudden and considerable manifestations of ozone which sometimes occur may be found to be due to descending currents bringing air from the cloud region to lower and warmer levels and thus causing the rapid evaporation of the condensed vapour which it contains. It may also enable us to trace out the causes of the differences which exist between the mean amounts of ozone in different localities, and will, I believe, be useful in suggesting new methods of treating meteorological observations and attempting the solution of some of the difficult but interesting problems which are at present engaging the attention of meteorologists.

February 29th, 1876.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the chair.

"An Account of some early Experiments with Ozone, and remarks upon its Electrical Origin," by J. B. DANCER, F.R.A.S.

Nearly a century since it was noticed that oxygen gas, when electrical sparks had been passed through it, acquired a peculiar smell and the power of attacking mercury. Somewhat later it was found that electrified air possessed the property of purifying decomposing animal and vegetable matter. In 1826 Dr. John Davy believed this principle existed in the atmosphere, and proposed tests for its detection.

When residing in Liverpool in the year 1838 I supplied some Daniell's batteries to Mr. T. Spencer. The apparatus consisted of a number of cells, in which I had substituted* thin porous jars, made of unglazed biscuit ware, to separate the two fluids, in place of the bladders, and ox gullets, which were very disagreeable in use, and easily destroyed; these porous jars and cells are now in general use, not only in Daniell's, but also in Grove's, and other batteries in which two fluids are employed.

It was expected that this battery would exhibit considerable power, and an evening was fixed upon to try some experiments with it at Mr. Spencer's house. The parties present at these experiments were Mr. Spencer, the late Mr. John Wilson (teacher of chemistry at the Liverpool Mechanics' Institution), Mr. James Robinson (now of Dublin), and myself. The first experiment was the decomposition of water. A Faraday's voltameter, with large platinum electrodes, was connected with the battery, and the mixed gases, as they were evolved, were collected in a glass gas jar, of the capacity of 180 cubic inches. The gas jar was filled with water, and placed on the shelf of a pneumatic trough; a bent glass tube from the voltameter had its end placed under the open bottom of the jar. When the experiment had proceeded for a short time we noticed that a white cloud, or vapour, escaped from the bubbles of

* Page 229. First edition of Elements of Natural Philosophy, by Dr. Golding Bird. London, 1839.

the mixed gases as they burst at the surface of the water in the jar. This white cloud remained permanent, and its unexpected appearance gave rise to many conjectures. When the jar became filled with the oxygen and hydrogen, it was removed from the shelf for the purpose of refilling with water, in order to repeat the experiment. As the gases were displaced from the jar, we became aware of the presence of a powerful odour, so pungent as to produce coughing. This was a second surprise, and it led to considerable discussion. The prevailing opinion seemed to be that some impurity in the sulphuric acid, used in the voltameter, had caused the white cloud and the odour. On one point we were all agreed—the odour was identical with that produced during the excitement of a frictional electrical machine, and the novel phenomena we had witnessed were admitted to be worthy of investigation. On repeating the experiment a second time similar results were obtained, and the room was filled with the electrical odour; being manipulator, I probably inhaled more of this odour than the rest of the company, and my throat was in a state of irritation for several days. I constructed another battery of similar power to that already named, and repeated the experiment at my own house, taking the precaution to have carefully-prepared sulphuric acid in the voltameter. The white cloud and odour were produced just as in the first experiments.

It became clear, therefore, that impurity in the sulphuric acid was not the origin of either cloud or odour. I filled several bottles with these odorous gases, and tried some experiments, such as the solubility of the odorous substance in water, its alkaline and acid properties, &c. These experiments were named to many scientific friends, one of them, the late Dr. Brett, then a chemist of repute, a relative of Dr. Golding Bird, of London. Dr. Brett offered to join me in investigating the properties of this peculiar odorous body,

and it was agreed that we should commence experimenting as soon as leisure would permit. Business affairs, however, obliged me to leave the subject in abeyance.

In the year 1839 F. C. Schönbein, Professor of Chemistry at Basle, whilst decomposing water by a voltaic battery, noticed that an odorous substance was evolved along with the oxygen gas at the positive pole. The identity of this odour with that which usually accompanies lightning, and that which is produced by passing sparks from an electrical machine to pieces of platinum or gold, induced him to enter on an investigation to discover, if possible, the source and properties of this odorous body, to which he gave the name "Ozone." His discovery was announced in a memoir which he presented to the Academy of Munich in 1840. When an account of his experiments appeared in the scientific periodicals, I recognised under the name of ozone the odorous body which had surprised us during our experiments in 1838.

The announcement of Schönbein's discovery caused considerable excitement in the scientific world, and a large number of scientific men in this country, and on the Continent, commenced investigating the properties of this remarkable body. In this brief and imperfect notice I can only name a few of the experiments. Those who are interested in the history of ozone may consult Dr. C. Fox's exhaustive work on ozone and antozone.* In "Nature," March 5th and 12th, 1874, there is also a very interesting account of ozone, in an address delivered before the Royal Society of Edinburgh by Dr. Andrews, F.R.S., December, 1873.

What is ozone? This is a question not easily answered in a satisfactory manner. This body has been tortured in every conceivable way by a legion of able experimentalists for the last thirty years, and up to this moment scientific men are not all agreed upon this subject.

* Published by Churchill. 1873.

Ozone is formed by passing an electric current through air or oxygen gas. Schönbein discovered that it was also generated, during the slow oxidation of phosphorus, in moist air. Ozone is a powerful bleaching and oxidising agent, possessing the property of purifying decomposing animal and vegetable substances. It oxidises many of the metals, is insoluble in water, and in solutions of acids and alkalies. If breathed in a concentrated state, it has a powerful effect on the animal system. To test the presence of ozone in the atmosphere, Schönbein proposed the employment of slips of paper impregnated with iodide of potassium and starch. These are exposed to the atmosphere with certain precautions; if ozone is present, these papers become coloured, and the depth of colour is supposed to indicate the amount of ozone. Schönbein at first supposed that ozone was a new electro negative element, analogous to chlorine and bromine; afterwards he suspected it to be a constituent of nitrogen. His investigations were continued for many years, and, from certain experiments, he concluded that oxygen could exist in three distinct states. When electro negative it was ozone, when positive it was antozone, and when these two states were combined it became passive, or neutral oxygen.

There are some scientific men who do not think the existence of antozone is satisfactorily proved. The elaborate researches of Dr. Andrews and Professor Tait have added largely to our knowledge of the nature and properties of ozone. These gentlemen discovered that when oxygen gas has been ozonised by a discharge of the electric current the volume of gas was diminished—for example, 100 volumes of oxygen become reduced to about 92 volumes. If changed back again by the agency of heat, or otherwise, it becomes 100 volumes again. Sorét discovered that the oils of turpentine and cinnamon, being brought into contact with ozonised oxygen, the whole of the ozone was absorbed He concluded that ozone was 1½ times the density o

oxygen. At present the prevailing opinion is that ozone is an altered condition of oxygen, the molecular constitution being different to that of ordinary oxygen.

Is ozone which is produced by an electrical discharge through air, or oxygen gas, identical with that observed in the atmosphere?

Many eminent men of science doubted the existence of atmospheric ozone; it was thought that other active substances in the air had similar reactions to those exhibited by ozone. The careful experiments of Dr. Andrews, which were intended to settle this important question, appear to confirm in a satisfactory manner the original views of Schönbein.

Ozone presented itself to my view in such close union with electricity that I have been almost forced to believe in its electrical origin. I expected that it would be ultimately found that an unstable chemical union existed between electricity and oxygen in the ozonised state. Some very careful experiments by Dr. Andrews appear to show conclusively that such is not the case; he finds that ozone may be readily destroyed by agitating it strongly with fine fragments of glass. Dr. Andrews remarks that this experiment forms a new and closer link than any hitherto observed between a purely mechanical action and a chemical change. Probably the generation of ozone is constantly taking place in the higher regions of the atmosphere; the formation of clouds, rain, hail, snow, &c., developing free electricity, under conditions highly favourable for the conversion of oxygen into ozone. The amount thus produced would be vastly augmented by agencies in operation at certain portions of the earth's surface. Ozone is said to be abundant at sea. This might have been anticipated; the friction of the wind on the surface of the waves, and the conversion of water into aqueous vapour (a change of state which always developes an abundance of free electricity) produce conditions as favourable as those

which exist in the upper regions of the atmosphere.

In the Lake district the manifestations of ozone are considerable; again there are conditions somewhat similar to those already named.

On land the amount of ozone generated will vary considerably with the character of the surface of the earth. In localities where vegetation abounds ozone should exist in considerable quantity; there is the mechanical action of the air over the moist surfaces of the vegetation, and the formation of aqueous vapour; and if it be true, as stated, that the oxygen which is evolved by plants is in an ozonised condition, we should have an additional source for the production of this purifying agent.

Heavy storms of rain, hail, and snow are always accompanied by free electricity and a manifestation of ozone. The pleasant sensations experienced on breathing the atmosphere after heavy rain are, perhaps, not altogether due to the washing of the atmosphere, but in part produced by the ozone contained in it. Mr. Baxendell noticed that when the fountains at the Waterworks, Arnfield, were set in operation he found the manifestation of ozone greater than he had observed in severe thunderstorms. This I can quite imagine possible. In discharges of lightning the production of ozone would be confined to the locality of the flash, and the effect on the test paper would depend on its situation and the direction of the wind. A water fountain may be regarded as a hydro-electric machine,* the friction of the water issuing through the jets developing electrical action, materially assisted by the conversion of the spray into aqueous vapour. I would suggest that this fact should be prominently brought before municipal bodies, to induce them to erect fountains in all available places in large cities as sanitary agents. They might prove highly beneficial in crowded localities.

* Humboldt mentions in Cosmos, vol. 1, page 344, "That the negative electricity near high water falls is sufficiently intense to produce an effect on a delicate electrometer at a distance of 300 or 400 feet."

"Results of Rain Gauge Observations, made at Eccles, near Manchester, during the year 1875," by THOMAS MACKERETH, F.R.A.S., F.M.S.

The rainfall of last year was above the average, by a little over the amount which fell in July above the average for that month. Hence it may be said that the excess of rainfall for the year was due to the excessive amount which fell during July. Great excesses fell during June and September, and the fall during all the summer and autumn months was invariably above the average. The fall was greatly below the average in February, March, April, and December, so that the year was characterised by a dry spring and a very wet summer and autumn, though the excess of the rainfall of the year is about the average fall for a month at this station, yet the number of days on which rain fell is below the average. This usually happens during the periods of excessive rainfall, which shows how much more rapidly the clouds condense during periods of heavy than of light rainfalls; and is another instance of a rule in rainfall that I pointed out last year. The following table shows the results obtained from a rain gauge with a 10in. round receiver placed 3ft. above the ground.

Quarterly Periods. Average of 15 years.	Quarterly Periods. 1875.	1875.	Fall in Inches.	Average of 15 years.	Differences.	Quarterly Periods. Average of 15 years.	Quarterly Periods. 1875.
Days.	Days.						
52	49	January	3·469	2·857	+0·612	7·336	5·081
		February	0·833	2·103	—1·270		
		March	0·779	2·376	—1·597		
46	44	April	0·763	1·913	—1·150	6·729	7·336
		May	2·676	2·116	+0·560		
		June	3·897	2·700	+1·197		
53	56	July	5·624	3·223	+2·401	10·692	15·180
		August	4·067	3·333	+0·734		
		September	5·489	4·136	+1·353		
58	56	October	5·030	4·310	+0·720	10·514	10·295
		November	4·099	3·325	+0·774		
		December	1·166	2·879	—1·713		
209	205		37·892	35·271	+2·621		

In the next table are given the results obtained from rain gauges of two different kinds, placed in close proximity in the same plane, and 3ft. from the ground, the one has a 10in. round receiver, and the other a 5in. square receiver. Nearly all the months on which there was an excess of rainfall the smaller gauge registered the larger amount; out of the four months when the rainfall was below the average three of them show the greater fall in the greater gauge. Nearly the whole difference between the fall in the two gauges occured in January. A similar circumstance happened in the December of the previous year, that is in the preceding month, and both cases were doubtless due to the same cause, namely the fall of snow. An average fall in both gauges over a period of eight years shows a difference of only $\frac{29}{1000}$ of an inch. Thus as I have said before the two gauges are practically checks upon each other.

1875.	Rainfall in inches in 10in. round receiver 3ft from ground.	Rainfall in inches in 5in. square receiver 3ft. from ground.	Difference.	From 1868 to 1875. Average of 8 years rainfall in inches in 10in. round receiver 3ft. from ground.	Average of 8 years rainfall in inches in 5in. square receiver 3ft. from ground.	Difference.
January	3·469	4·062	—·593	3·082	3·142	—·060
February	0·833	0·824	+·009	1·974	1·940	+·034
March	0·779	0·724	+·055	2·203	2·227	—·024
April	0·763	0·779	—·016	1·853	1·827	+·026
May	2·676	2·690	—·014	2·008	1·978	+·030
June	3·897	3·956	—·059	2·548	2·522	+·026
July..............	5·624	5·710	—·086	3·148	3·143	+·005
August	4·067	4·056	+·011	3·372	3·351	+·021
September......	5·489	5·497	—·008	4·069	4·030	+·039
October	5·030	4·960	+·070	4·983	4·967	+·016
November	4·099	4·118	—·019	3·272	3·306	—·034
December	1·166	1·131	+·035	3·023	3·073	—·050
	37·892	38·507	—·615	35·535	35·506	+·029

In the next table I give the results obtained from two exactly similar gauges placed at different heights from the ground and free from every interference; each gauge has a 5in. square receiver, and the one is placed 3ft. and the other 34ft. above the ground. The total fall in the one 3ft. from

the ground for last year was 38·507in., and in the one 34ft. from the ground it was 32·921in. The difference between the fall in the two gauges is 5·586in. or about 18 per cent less rain fell in the higher gauge than in the lower one. In the same table I give the average fall in the same gauges for eight years, and by comparing the results it will be found that the average difference between the fall in the two gauges is about 17½ per cent, very nearly the difference I showed last year on a seven years' average.

1875.	Rainfall in inches in 5in. square receiver 3ft. from ground, 1875.	Rainfall in inches in 5in. square receiver 34ft. from ground, 1875.	From 1868 to 1875. Average fall of rain in inches for 8 years in 5in. square receiver 3ft. from ground.	From 1868 to 1875. Average fall of rain in inches for 8 years in 5in. square receiver 34ft. from ground.
January	4·062	2·618	3·142	2·217
February	0·824	0·585	1·940	1·461
March	0·724	0·605	2·227	1·748
April	0·779	0·543	1·827	1·545
May..............	2·690	2·345	1·978	1·784
June	3·956	3·326	2·522	2·220
July	5·710	5·312	3·143	2·819
August	4·056	3·791	3·351	2·852
September	5·497	4·917	4·030	3·453
October	4·960	4·605	4·967	4·154
November	4·118	3·464	3·306	2·597
December	1·131	0·815	3·073	2·460
	38·507	32·921	35·506	29·310

The following table gives the ratios of the excesses of rainfall 3ft. from the ground over the amount measured at 34ft. from the ground. It is astonishing how these ratios vary their places in each single year, and yet how they maintain their positions in and after a six years' average. There was absolutely no comparison between the ratios of the single years of 1873 and 1874 and the ratios of the six and seven years' averages, which were almost identical. So also of these ratios of last year, there is not the slightest comparison between them and the eight years' average, and practically there is no difference between the six, seven, and eight years' averages. Now, according to the seven and

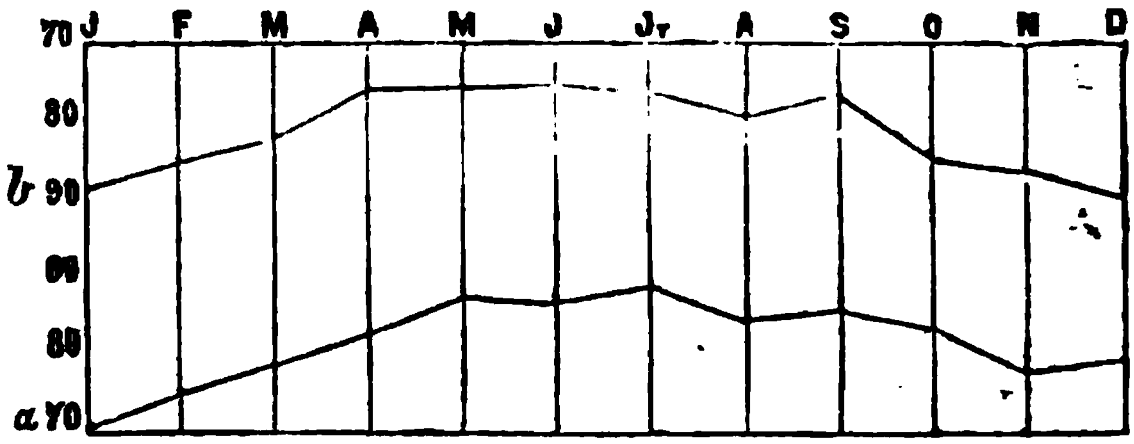
eight years' averages, the greatest difference between the amounts of rain which fell in the lower and higher gauges occurs in January, decreases gradually in difference till May, when it attains a minimum. This minimum maintains itself till August, when there is a slight increase; then after a slight decrease in September, the increase becomes constant to January again. On the theory first enunciated by Mr. Baxendell that the excess of rainfall in the lower gauge is due to the particles of invisible vapour in the air between it and the higher gauge coalescing with the falling rain-drops, the results seem to show that in the spring and early summer months there is relatively less of this vapour in the air below a height of 34ft., and there is relatively more of it in the winter months and particularly in January.

Monthly and annual ratios of the excess of rainfall measured at 3ft. from the ground over the amount measured at 34ft. from the ground; together with the amount of the mean humidity of the atmosphere, full saturation being represented by unity.

	Ratios of such rainfall for 1875.	Ratios of such rainfall for an average of 8 years, from 1868 to 1875.	Mean humidity of the atmosphere for 8 years, from 1868 to 1875.
January	·643	·705	·871
February	·709	·758	·855
March	·835	·784	·836
April	·697	·845	·765
May	·871	·901	·755
June	·840	·879	·747
July	·930	·896	·759
August	·934	·851	·793
September	·894	·856	·777
October	·929	·836	·840
November	·841	·785	·853
December	·720	·800	·872
Annual Ratios	·820	·824	·810

That I might demonstrate as far as possible that this is the true way to account for the difference of rainfall at the two heights above the ground, I took out the representatives I had for the last 8 years of the relative amount of moisture in the atmosphere. These results were obtained from

wet and dry bulb hygrometer, which I have had in use for 16 years, and reductions based upon Glaisher's humidity tables. In the foregoing table the averages of the relative amount of moisture in the atmosphere are given in the last column. Of course the order of the amounts is inverse to the ratios of rainfall, one being positive and the other negative. The striking resemblance between facts shown by each is at once obvious. I have, however, projected them in the following diagram. The irregular line marked



(*a*) represents the rainfall ratios, and the one marked (*b*) the mean relative amount of humidity. Hence I infer that the maximum of dry air on the ground is in May, June, and July, and the minimum in November, December, and January. These periods of maximum and minimum are closely allied to similar periods of ozone results.

In the next table I give the fall of rain for 1875 during the day, from 8 a.m. to 8 p.m., and the fall during the night from 8 p.m. to 8 a.m. The results of these observations for 1874 I pointed out were exceptional, as in every previous year since I instituted them the day-fall had always been greater than the night-fall. The last year gives the original kind of results, and the day-fall exceeds the night, and by about the same amount as the reverse of 1864. The excess of the day-fall over the night of last year was 2·517 inches, or about 13 per cent.

1875.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Difference between night and day fall.
January	1·605	2·457	+0·852
February	0·468	0·356	—0·112
March	0·380	0·344	—0·036
April	0·396	0·383	—0·013
May	1·249	1·441	+0·192
June	2·425	1·531	—0·894
July	3·780	1·930	—1·850
August	2·033	2·023	—0·010
September ...	2·410	3·087	+0·677
October.........	2·759	2·201	—0·558
November ...	2·584	1·534	—1·050
December......	0·423	0·708	+0·285
	20·512	17·995	—2·517

In the next table I present the average day and night fall for a period of eight years. The results of this table continue to show that the day-fall exceeds the night-fall so far as the whole year is considered. The months which have an excess of rainfall in the nights are those which show the greatest amount of atmospheric vapour, namely January, February and December; August and September show an increase, but in these months the amount of vapour in the air begins to increase, but whether the excess in those months is due to that cause is yet doubtful.

Average of eight years, from 1868 to 1875 :—

1875.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Difference between night and day fall.
January	1·361	1·781	+0·420
February	0·825	1·114	+0·289
March	1·199	1·027	—0·172
April	1·054	0·772	—0·282
May	1·156	0·821	—0·335
June	1·463	1·058	—0·405
July	1·813	1·330	—0·483
August	1·656	1·695	+0·039
September ...	1·875	2·155	+0·280
October.........	2·585	2·381	—0·204
November ...	1·682	1·624	—0·058
December......	1·351	1·721	+0·370
	18·020	17·479	—0·541

MICROSCOPICAL AND NATURAL HISTORY SECTION.

March 13th, 1876.

Professor W. Boyd Dawkins, F.R.S., F.G.S., in the chair.

Mr. John Boyd was elected a Member, and Mr. Robert Ellis Cunliffe, and Mr. Walter Edward Barratt, Associates of the Section.

Mr. Charles Bailey exhibited a series of slides illustrating similarities of structure in Dicotyledonous and Monocotyledonous stems.

Mr. Bailey likewise distributed among the members dried plants of Potamogaton lanceolatus sm. from Lligwy, Anglesea, that locality being still the only recorded habitat for this rare Pondweed; and exhibited some Egyptian plants collected by H. A. Hurst, Esq., Treasurer of the Section—the most noteworthy of which was Tamarix articulata—from Alexandria.

Mr. R. D. Darbishire, F.G.S., exhibited a series of specimens of very young Rhombus vulgaris (Cuv.), showing (1), the two eyes on each side of the vertebral plane, (2) the removal of the eye from the underside to the dorsal edge, (3) the appearance of both eyes on the one (upper) side of the fish. These specimens had been found and given by Dr. A. W. Malm, of Gothenburg; who first proved the remarkable physiological change of form. Dr. Malm stated that he had had no difficulty in procuring specimens so young as these by using a surface net at sea before dawn in the hot days of summer. These were taken in the Kattegat on the 25th July, 1875.

Mr. R. D. Darbishire, F.G.S., also communicated some notes made during a visit in the past summer to the Swedish shell-beds of Uddevalla and the neighbouring district, and exhibited a collection of the fossils of remarkable extent and beauty. He referred to the notices of these fossils by Linnæus, 1747 (9 species), and others; by Sir C. Lyell, 1834, in his paper on the proofs of a rising of the land in Sweden (Philos. Trans. 1834) (25 species); by Mr. J. G. Jeffreys, 1863, British Ass. Rep. (85 shells, and 14 other invertebrata), and M. Thudén, 1866 (116 specimens of shells), and to the great collections of Mr. R. Thorburn in the Museum of Uddevalla, and Dr. Malm in that at Gothenburg. The two latest lists must be used with discrimination as they are conchological rather than geological; each enumerating in one consecutive list fossils from older, newer and comparatively recent beds.

He described the great deposits at Kapellebacken S.W. and at Samneröd, Bräcke and Kurod, N.E. of Uddevalla, and noted especially the following facts as proving that, whether the greater part of those vast accumulations of remains had been drifted into the bottom of the ancient fiords or not, some at least of the shells had lived where they are now found. The perfect and unviolated occurrence of the species named, the horizontal position of the small beds of sand and shingle in which some of the shells are found, point to very steady and slow movement. Sir Charles Lyell indicated the present rise of the southern part of Sweden as at the rate of 3 feet per century. As the highest beds at Uddevalla are about 206 (English) feet above the level of the sea, this means that they have been rising for at least 7,000 years. How long it was before they emerged from the water, that the lower parts of the deposit, which at Kapellebacken are apparently more than 70 or 80 feet thick, were laid in the bottom of the sea, there is no record. The most elevated beds of the district appear to be those at

Kapellebacken. In a small quarry, where the material has been worked for agricultural purposes, the following shells were found under conditions which imply that they lived where they now lie. They occur in horizontal patches, shewing on the sections as level, and remarkably limited strata. There is, at a level many feet below the top of the older deposit, a curious patch of much more modern raised beach with Littorina littorea and Cardium edule.

This bed also is horizontal and undisturbed.

Mya truncata: The two valves together, upright in sandy shingle, and filled with the same.

Mytilus Edulis: In beds, the valves of various sizes lying confused and packed. In this species the ligament and hinge are weak and the valves speedily detach themselves. One pair was found in the mouth of a baccinum, which could scarcely have rolled over after its lodger had died without losing his shell.

Modiola Modiolus: In a bed of fine sand shells of this species occurred in a remarkably perfect condition, the valves closed, posterior edge uppermost, the hinge and back at the top; large shells and smaller (all of a more fragile variety than is commonly dredged in British seas.) In digging back into this layer shell after shell appeared; while fresh, "as perfect as if alive," but soon to warp and crack in drying.

[Query: Does this Modiola ever live burrowing free in fine sand ?]

Pecten Islandicus: In beds, large and small, the valves together, horizontally, with the flatter valve uppermost. The colour of this shell is often preserved with extraordinary freshness.

Buccinnum undatum: and B. Greenlandicum: Both species occasionally occurred, older and younger shells together, grouped in horizontal strata, though, naturally, not so massed as the less locomotive bivalves, and besides, generally diffused through the whole deposit.

Fusus Antiquus: Also occurred, though not massed, old and young shells free from sea-wear.

Echinus Dröbachiensis: Also occurred in sandy layers, grouped, old and young, as these animals are usually found to live.

Balanus Hameri, a species whose component plates seem very slightly attached to each other, and speedily fall apart after death and disturbance, occurs whole. It was at this locality that M. Brongniart noticed the basal plate shells adhering to the gneiss rocks, between which the shales are heaped. The like were still to be found there.

In the more amply extended deposits at Bräcke, which do not rise above a level of 100 feet above the sea, there are enormous accumulations of the shells of Saxicava rugosa. Wherever there was a fresh section ready, or could be made, it was easy to pick out multitudes of specimens with the two valves together. As the ligament of this shell is very slight, and the hinge nothing, these pairs of valves cannot have suffered either sea wash or any geokinetic change of position, or even pressure.

The great size and the freedom of growth of the Uddevalla Saxicava has been long noticed, indicating its life outside of such burrows as gave the more modern form its name. One may suppose that the shells, growing at liberty amongst piles of shells of their ancestors, may well have developed, as we see. Both varieties (rugosa and arctica) occur, the latter somewhat more rarely.

At the same place there were found amongst the Saxicava valves, which form, as it were, the matrix of the deposit, many specimens of *Astarte borealis,* old and young, with both valves in juxtaposition, placed vertically with the posterior end uppermost, in the position of life. *Astarte elliptica* occurred also in the same condition, though more rarely.

It was at Bräcke that Sir Charles Lyell found the balanus hameri still attached to the rocks.

Saxicava arctica, Mya truncata, and Mytilus edulis appear in forms peculiarly distorted, in a way which it is usual to attribute to exposure, during intervals, to greater or less infusion of fresher or colder water. This may be supposed to be due to some direction of the currents of the old land streams, probably coming down from glaciers, over shellbeds, not far from shore, and at no great depth. The variation of Mya truncata from some such cause has given rise to the form known as M. Uddevallensis.

Dr. Thomas Alcock has kindly examined three parcels of sand, and reports finding the following species:

	Kapellebacken—high level·		Bräcke—lower. From sand among shells.
	From sand in Modiola.	From sand in Echinus.	
Polystomella umbilicatula..	*		*
Miliolina seminulum	*	*	*
„ trigonula............			*
Rotalina turgida (?)	*	*	*
Truncatulina lobatula		*	*
Another form			*
Mollusca, fry of		*	
Ostracoda	3	2	3

The specimens are all of strong kinds, and appear much worn. They were not common in any parcel.

Mr. John Plant made the following remarks, being addenda and corrigenda to the list of shells already published, found in Cymmeran Bay, Anglesea.

List of Shells found in Cymmeran Bay, Anglesea. Corrections and Additions, by John Plant, F.G.S.

Since the printing of my last list of the Fauna of Cymmeran Bay, the collection of shells obtained from there has been more than once revised, and I have seen reason to believe that in several instances my determination of the species has been faulty, from my having had only

young or greatly worn shells to deal with. Several species have been added to the list of this collection. It is therefore necessary to print a few *corrigenda* and *addenda*.

25. Lucina leucoma, was so named from a few worn and small valves, since which living shells have been taken and it proves to be L. borealis, smaller than normal specimens.

31. Cardium rusticum; this must be given up as belonging to this locality. All my specimens belong to C. echinatum.

96. Venus casina, are shells much worn of V. verrucosa.

97. Circe minima; the one small valve so named proves to be a very young Cyprina Islandica.

99. Cardium fasciatum; the two small specimens so named are young of C. cchinatum.

109. Pecten niveus; the valve so named is a white variety of P. pusio, a common shell in the bay. White varieties of P. varius approaching nearly to the niveus are also found.

39. Acmæa testudinalis; the shells so named are the young of a variety of P. vulgata.

121. Assiminea Grayana are only worn and small forms of Lacuna Vincta and L. puteola.

Fusus Islandicus should be called F. gracilis, the former name has long been applied to this English shell, but erroneously.

The following are additional species which have been satisfactorily determined since the last list was printed.

Venus ovata, several specimens from both Cymmeran and Holyhead.

Artemis lincta, a iew valves.

Cardium echinatum, previously named C. rusticum.

Cardium pygmæum, not very common.

Astarte sulcata, one small specimen.

Lucina borealis, previously named L. leucoma.

Modiola modiolus, not common.

Sphænia Binghami, a good number of smallish specimens.

Trochus Lyonsii, a well marked variety of T. zizyphinus.

Dentalium tarentinum, from Holyhead.

Patella athletica, rather common.

Natica nitida, far from common.

Odostomia acuta (Jeff.), two specimens of a dull white variety, distinctly umbilicated—vide Brit. Mol. v. 3, p. 270.

Sepia bisserialis, Towyn Capel, Defarh bays. Mrs. Plant found four of these beautiful and rare shells after very stormy weather, in August, 1873.

Ordinary Meeting, April 4th, 1876.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

Professor W. Boyd Dawkins, F.R.S., called the attention of the Society to the depreciation of silver which is now under the notice of a select committee of the House of Commons. It has been attributed to a panic, to the demonetisation of silver in Germany, or to the increased production of silver. In all probability all these causes have been in operation together. With regard to the last he had had the opportunity of examining a part of the silver mining district in Nevada last autumn, and he was very much impressed by the enormous mineral wealth of that region, which is as yet scarcely touched. In spite of the depression of trade, which was marked by the number of miners out of work, new localities are being discovered which will afford an almost inexhaustible supply of silver. To take an example. In June last a new lode was discovered in the range of metamorphic schists and slates about 12 miles from Battle Mountain, a station on the Central Pacific Railway, 519 miles from San Francisco. When he visited it in October it was nearly in working order, and at the present time is in full production. The lode runs N. and S. and outcropped on the surface of the ground, forming a vertical mass looking almost like a broken down wall in some places, and measuring 32 feet wide, the richer portions being of course irregular in thickness.

These metalliferous ranges extend southwards through Arizona and New Mexico into the great Mexican mining districts, and northwards to an unknown limit. The principal obstacles to their wealth being realised are, 1. The hostility of the Indians, 2. The want of wood, 3. The difficulties of carriage. These however are swiftly being removed. The Indians are rapidly perishing, and the railways are bringing places hitherto inaccessible within reach of the seaboard and the Eastern States. And although the lonely plains covered with sage-brush which sweep round the metalliferous ranges almost like a sea, and at a height of from 4000 to 5000 feet, are without a tree, the Sierra Nevada on the one hand, and the Rocky Mountains on the other, offer an endless supply of timber. It seems therefore that the production of silver in this district is likely to be largely increased. Unless the demand keep pace with the supply, the price must necessarily fall. Fourteen tons of silver coined at the San Francisco mint were sent to the Eastern States on 21st of March last, and the export from San Francisco to China, which was under one million sterling in 1874 according to Sir Hector Hay, in 1875 reached one and a half millions, which gives an increase of 50 per cent, and this in spite of the extraordinary depression of trade.

"On some Isomerides of Alizarine," by EDWARD SCHUNCK, Ph.D., F.R.S., and HERMANN ROEMER, Ph.D.

Considering the importance of everything connected with the history of alizarine, we have been induced to undertake the study of such of the isomerides of that body as we have been able to obtain. These isomerides are interesting from a theoretical point of view, as presenting a problem with regard to internal constitution which has not yet been solved, and technically some of them are interesting as they occur along with artificial alizarine and not being available for tinctorial purposes are the source

of loss to the manufacturer. The isomerides of alizarine hitherto observed are the following:—

1. *Purpuroxanthine*, a body first obtained by Schützenberger from commercial purpurine, and afterwards prepared artificially by the action of reducing agents on purpurine. It crystallises in yellow needles, soluble in alkalies with a blood-red colour.

2. *Isoalizarine*, a substance derived from madder and described by Rochleder, having properties very similar to those of purpuroxanthine, and perhaps identical with it.

3. *Frangulic Acid*, a substance also very similar to purpuroxanthine, obtained by Faust by the decomposition of franguline, a constituent of the bark of Rhamnus frangula.

4. *Anthraflavic Acid* or *Anthraflavine*, a body accompanying artificial alizarine, first described by one of the authors in a paper read before this Society,* and subsequently examined by Mr. Perkin. Its isomerism with alizarine was established by Mr. Perkin, who was the first to obtain it in a state of perfect purity. It is easily distinguished by the colour of its alkaline solution, which is yellow.

5. *Anthraflavon*, a product obtained by the action of diluted sulphuric acid on oxybenzoic acid. We have not yet had an opportunity of preparing and examining this body, but on reading the description of it given by its discoverers, Barth and Sennhofer, it is evident that it bears a strong resemblance to the preceding.

6. *Quinazarine*, obtained by Baeyer, by the action of phthalic acid on hydroquinone. Of all the isomerides it most resembles alizarine itself. Its alkaline solutions have the same violet colour as those of alizarine, and it dyes mordants, while the other isomerides have no tinctorial properties.

7. *Chrysazine*, a body formed by the action of nitrous acid on hydrochrysammide, and carefully examined by its

* Memoirs, 3rd Series, Vol. V., p. 227.

discoverer, Liebermann. By the action on it of strong nitric acid it yields chrysammic acid, the nitro-acid first obtained from aloes by one of the authors many years ago.

To these we have now to add—

8. *Isoanthraflavine*, a substance accompanying artificial alizarine, generally found along with anthraflavine in the commercial product, and which we shall describe presently.

Chrysophanic Acid, the crystalline colouring matter of rhubarb, which at one time occupied a place in the list has been erased, since it has been shown by Liebermann that it is in reality a homologue of alizarine, having the formula $C_{15}H_{10}O_4$, and is derived not from anthracene, but from a methylanthracene.

We propose in this paper to give an account of some experiments on two of these isomerides, viz., anthraflavic acid and the one generally accompanying it, which we have lately observed for the first time.

Anthraflavic Acid or Anthraflavine.

We have little to add to the description of this substance given in the paper above referred to. Its melting point is above 330°C. It is less soluble in glacial acetic acid than in alcohol. The analysis of a carefully purified specimen of the substance gave numbers agreeing exactly with the formula $C_{14}H_8O_4$, and confirming the results obtained by Perkin. The barium salt, which has been previously described, loses when dried over sulphuric acid a considerable quantity of water, becoming at the same time much lighter in colour. On being now heated at a temperature of 150°—180°C. it loses two molecules of water, and the dried salt has a composition corresponding with the formula $C_{14}H_6BaO_4$. Our results do not quite agree with those of Perkin, who found the formula of the salt dried at 180° to be $2C_{14}H_6BaO_4$, H_2O.

Tetrabromanthraflavine, $C_{14}H_4Br_4O_4$, is prepared by adding bromine in excess to an alcoholic solution of the

substance. It crystallises in yellow needles, which are almost insoluble in the usual menstrua, such as alcohol, benzol, and glacial acetic acid.

Nitroanthraflavic Acid, a body already referred to in the paper of 1871, is prepared by dissolving anthraflavine in fuming nitric acid and after allowing to stand some time, adding water, which precipitates the nitro-acid as a light yellow crystalline powder. It is obtained on spontaneous evaporation of its alcoholic solution in large well-defined rhombic crystals of a deep yellow colour, the composition of which is expressed by the formula $C_{14}H_4(NO_2)_4O_4$. Most of the salts, such as the potassium, sodium, magnesium, barium, silver, and mercury salts, are soluble in boiling water, and crystallise in lustrous needles, varying in colour from light-yellow to brownish-red. By reduction with tin and hydrochloric acid the nitro-acid yields a dark-blue powder, which is almost insoluble in alcohol, glacial acetic acid, &c., but dissolves in caustic alkalies with a fine violet colour like that of alkaline solutions of alizarine.

Diacetylanthraflavine has already been described by Perkin.* We found its melting point to be at 227°C.

Diethylanthraflavine, $C_{14}H_6(C\,H_5)_2O_4$ was prepared by heating a mixture of anthraflavine, caustic soda, iodide of ethyl, and a little alcohol in sealed tubes to 120°, and crystallising the product from boiling alcohol. It crystallises in light yellow needles, which are soluble in benzol and glacial acetic acid, but insoluble in water. It fuses at 232°. The fused substance on cooling is converted into a mass of prismatic crystals. The spectrum of the solution in concentrated sulphuric acid, which is red, shows a well-defined absorption band between the green and blue.

Dimethylanthraflavine, the preparation of which is similar to that of the preceding, has almost the same properties as the ethyl compound. It fuses at 247°-248°.

* Journal Chem. Soc., XXVI., p. 20.

Isoanthraflavine.

This isomeride of alizarine was prepared from a by-product of the manufacture of alizarine supplied to us some time ago by Mr. Perkin, and which, according to the latter, had been obtained by treating the crude alizarine with lime-water, filtering and precipitating the red extract with acid. The product was treated with dilute caustic soda lye, in order to separate some anthraquinone. The filtrate gave with hydrochloric acid a yellow gelatinous precipitate, which was filtered off and treated with cold baryta water, until nothing more dissolved. The residue left undissolved after this treatment consisted of barium anthraflavate, and was employed for the preparation of anthraflavine. The blood-red solution was mixed with hydrochloric acid, which gave a yellow precipitate consisting of isoanthraflavine. This was purified by repeated crystallisation from boiling alcohol, and was obtained in long yellow crystalline needles. Sometimes it yielded golden yellow lustrous scales, but these on recrystallisation always gave needles. These needles, after drying over sulphuric acid, still contain one molecule of water of crystallisation, which is driven off by heating to 120°. The dried substance has a composition agreeing with the formula $C_{14}H_8O_4$, five analyses giving as a mean C69·79, H3·65, the calculated amounts being C70·00, H3·33. The properties of isoanthraflavine resemble those of anthraflavine. It melts at a temperature above 330°. When slowly heated between watch-glasses it yields a sublimate consisting of lustrous bright yellow needles and plates. It is a little more soluble in boiling water than anthraflavine. It dissolves easily in boiling alcohol and in hot concentrated sulphuric acid, but is almost insoluble in benzol and chloroform. It imparts no colour whatever to mordants and differs in this respect very widely from alizarine. It may be easily distinguished from anthraflavine by the colour of its alkaline solutions, which is distinctly red, while the

colour of anthraflavine solutions is deep yellow, or when concentrated reddish-yellow. In concentrated sulphuric acid isoanthraflavine dissolves with a cherry-red, anthraflavine with a yellow colour. The two substances may also be readily distinguished by their behaviour towards lime and baryta water, in which isoanthraflavine dissolves easily in the cold, yielding red solutions. Anthraflavine, on the other hand, is almost insoluble in cold baryta water, and only dissolves on boiling, while in lime water it is almost insoluble at all temperatures. Isoanthraflavine in most of its properties approaches purpuroxanthine even more closely than it does to anthroflavine; but having prepared a specimen of purpuroxanthine according to Schützenberger's process, we are enabled to assert positively that the two bodies are not identical. One of the characteristic properties of purpuroxanthine is that it yields phthalic by oxidation with nitric acid, whereas isoanthraflavine gives with nitric acid a nitro-substitution product.

The barium compound of isoanthraflavine can be made to crystallise (though not without some difficulty) in dark red needles resembling barium anthraflavate. It contains water of crystallisation, which it loses on being heated to 150°. The composition of the dry salt corresponds with the formula $C_{14}H_6BaO_4$.

Tetrabromisoanthraflavine, $C_{14}H_4Br_4O_4$, is prepared in the same way as tetrabromanthraflavine. It crystallises in yellow needles, soluble in boiling alcohol and in glacial acetic acid.

Diacetylisoanthraflavine, $C_{14}H_6(C_2H_3O)_2O_4$ was obtained by the action of acetic anhydride on isoanthraflavine at 160 to 180°. It crystallises in light yellow microscopic needles, which are soluble in alcohol and more easily soluble in glacial acetic acid. At 175° it commences to soften, and at about 195° it fuses completely. It is decomposed by alcoholic potash solution.

Diethylisoanthraflavine, $C_{14}H_6(C_2H_5)_2O_4$, was prepared in the same way as diethylanthraflavine, which it closely resembles. It crystallises from alcohol in long light yellow shining needles, soluble in alcohol and ether, more soluble in glacial acetic acid and benzol. It fuses at 193—194°. It dissolves in concentrated sulphuric acid, forming a purple solution, the spectrum of which shows two ill-defined absorption bands, one in the yellow, the other in the green.

Isoanthraflavine gives with fuming nitric acid a nitro-substitution product similar in its appearance and general properties to nitro-anthraflavic acid, but we have been unable from want of material to examine it minutely.

We will conclude with a few remarks on the action of caustic alkalies on anthraflavine and isoanthraflavine. On a former occasion it was stated by one of us that anthraflavine yields by the action of fusing hydrate of potash alizarine, and it was this supposed convertibility into alizarine which led Liebermann and others to the conclusion that anthraflavine was identical with monoxyanthraquinone. We are now in a position to assert with confidence that the product of the action of alkalies is not alizarine. On repeating the experiment on a larger scale by fusing the substance with caustic potash in a silver basin we obtained a substance which after being freed from impurities crystallised from alcohol in orange-coloured needles strongly resembling but certainly not identical with alizarine. On heating it yields a sublimate in needles very similar to sublimed alizarine. The solution in alkalies is however devoid of the violet tint characteristic of alizarine, and on dilution appears distinctly red. The spectrum of the solution shows two absorption bands similar to those of alizarine solution, but these bands, according to the determination kindly undertaken for us by Dr. Schuster, lie further away from the red end than the bands of alizarine. It appears probable that this body may turn out to be an isomeride of purpurine, resem-

bling Mr. Perkin's anthrapurpurine. Isoanthraflavine when treated in the same way yields a body which has most of the properties of anthrapurpurine, though it seems to crystallise more readily and in longer needles than the latter substance does, according to Mr. Perkin's account. We are at present engaged in the investigation of these products.

Professor BOYD DAWKINS, F.R.S., made the following remarks:

It is not my intention to add more than a few necessary words to the literature, serious and comic, which has Windy Knoll for its centre. At the last meeting I was unable to give a decided 'yes' or 'no' to Mr. Plant's evidence that the 'bone of contention' was not that of bison, because it is a fragment of a variable and unimportant bone which I had not seen for nearly two years. I have re-examined the evidence, and consulted Mr. Davies, of the British Museum, and I find that I was mistaken in referring it to bison. The mistake is however a side issue and not of any scientific importance, because the remains of that animal from that place are considerably over one thousand in number, while those of the bear are over sixty,

With regard to the real point at issue, as to whether it adds the cave-bear to the fauna of Derbyshire, as urged by Mr. Plant, I have only to add to the opinion expressed in Proceed. of Lit. and Phil. Soc., Manchester, 1874, p. 6, the views of Professor Busk and Mr. Davies. The former writes—"I should be very loth to give any opinion whatever as to the specific characters of a fossil bear from that part of the skeleton alone." The latter, after regretting that Mr. Plant had omitted to mention the discovery of the associated ursine jaws and teeth when the bone was submitted to him, says (25th March, 1876), "I have not sufficiently studied the bones of the bears, with the exception of the jaws and teeth, as to enable me to determine with certainty the species to which they belong. Had I known

all the facts, I should not only have hesitated but refrained from assigning the bone to any particular species of bear." The evidence of the jaws and teeth proves that the bear of Windy Knoll is not the cave, but the great fossil grizzly bear (*U. ferox fossilis=U. priscus*), as may be seen by a reference to the Quart. Geol. Journ., Lond. 1875, pp. 251-2. It is unnecessary to go into further details.

"The Eucalyptus near Rome," by Dr. R. ANGUS SMITH, F.R.S., V.P.

A few years ago I wrote to a friend in Italy, suggesting that, as he had leisure, he might try the value of the Eucalyptus in that country for the removal of those conditions which engender malaria. His reply was somewhat to the effect that every one was aware of the value of the tree, and everywhere trials were being made, so that there was no use in his doing what everybody was doing. This was very cheerful, showing how rapidly the Italians had risen up to a full appreciation of new ideas. This winter I went to Rome with a desire to learn on the spot something of the conditions of the ground complained of, and receive some knowledge from the learned men of the place, as well as from any books which may not be well known here. I certainly did not obtain any confirmation of the opinion that every one was attending to the subject. I saw men in scientific and social positions, in which one might expect to find the fullest knowledge of the subject, and I came to the conclusion that it had excited very little interest in Rome or the neighbouring country.

One small experiment, however, was spoken of, and several knew of it, although I met few who had seen it. It was made at the Church of St. Paolo alle Tre Fontane (usually called *Tre Fontane*), three or four miles from Rome, and as this is the only place I have seen from which to derive a lesson, I must be excused taking such a small

example. The church has two other churches near it, and a small residence for monks of the order of La Trappe. The station had been deserted for 40 years, and the ancient building—the largest and lowest in situation—had been filled up with mud to the depth of three feet. The Campagna here is much exposed to malaria, and hence the desertion. It is by no means a dead level, neither is the upper part of the Campagna or agro Romano by any means so. The soil is very deep, and it is cut into vales of a depth which I will not venture to characterize generally, but enough to say, they were at this place 30 to 40 feet. The streams, like the Tiber itself, had brought down their mud, and the place had become to a large extent a desolation. In 1869 it was re-inhabited; and before entering on the dwellings, a number of prisoners were set to clean out the place, remove the accumulated mud, and make drains. Round the desolate spot there is now a garden, and although it is not yet in fine order, it is, at least in sunshine, a great improvement on the neglected land around. I venture to give no exact history, but tell only such few things as were told me at the spot. During the first season several of the monks died—I think five was the number given—and of fever such as marshes produce. A few specimens of Eucalyptus Globulus were planted, and, as they grew well, a small garden is now thinly covered over with various species. As one enters there is a peculiar odour perceptible, it is fragrant, pleasant, and resinous; some compare it to that from turpentine, some to the black currant, but every one attempts to give the name of some other odour as evidently mixed with this more prominent one. The most of the plants are two or three years old and about ten feet high; but there is a diversity, and the oldest has been planted only five years. This largest was judged by myself and others to be about thirty-five feet high: at the height of fourteen inches from the ground it was eight inches in diameter.

Not one of the monks had died after the first year, but then the place had not been inhabited during the night until last summer. Still, it will be seen that the experiment is a very small one and a very imperfect one. It is, however, important in this respect, that during the last summer and autumn—the dangerous seasons—no one had died, whilst during the first there were several deaths. But it must be remembered that the place had been cleaned out and drained, and placed, as the buildings were, in the lower part of a valley, draining must have been much required, and the fact of mud being in the church itself, showed how readily the whole was flooded. Although there are now some trees growing, they are only at one side and they are small. It is true they give out a very distinct odour very striking when one goes near, and it may be said that the cause must pervade all the surroundings even if it be not perceptible. This will certainly take place in very still weather, and the low situation protects it greatly from winds. Still, with every desire to give to the emanations of the Eucalyptus every virtue demanded, it is not easy to look on this as a good instance of its success.

One of the monks by name Orsise, had prepared a tincture from the leaves, and a glass of this was given to every one daily when fever showed itself. This seems to have been the really efficient agent that in conjunction with the drainage protected the brethren last summer.

It is not my intention to speak of this substance with details to any extent. It is however known that in the leaves and the bark and even wood of the blue gum tree is an oil with a very strong odour. There may rather be said to be several oils, but one which has been called eucalyptol boils between 170° and 178° C. This is the oil which is said to resemble cajeput oil, and is said to have an effect similar to that of quinine. But the substance which has the medical effect is probably a much more volatile oil continually

rising from the plant, and J. Bosisto adds a volatile acid.*

This experiment mentioned shows that men may live in health in one of the worst parts of the Campagna with proper precautions, and how different would things be if this plan were general; instead of a neglected country with scarcely a house, it might be a pleasant habitation, as it once was, for many thousands. Still we should not expect this to occur if the inhabitants were obliged to take tincture of eucalyptus leaves very frequently. Indeed the use of the tincture is itself an objection to the experiment. The difficulty into which Sir Samuel and Lady Baker got when detained in Africa was aggravated by fever of a marsh kind, but seems to have been entirely removed when they used the spirit from the sweet potato. The fever was not a severe one, but the fact injures to some extent the experiment at Tre Fontane.

If however we look to other countries we are informed that the tree itself with its exhalation is quite sufficient to render a district healthy, and it is perfectly certain that if the oil is efficacious, and the evidence gives faith, those who live near must be continually taking in doses which must soon equal in amount that usually given as a cure. They must in fact be living in a constant vapour of this healing oil. In order to produce this condition the houses must be surrounded with the trees, and every wind will then blow its measure of health. At Tre Fontane this is not attempted, and although more ground has been promised the monks, it is not promised to extend on every side so that they may manage to be surrounded.

In speaking of the subject it of course occurs to ask: Is it really certain that this tree will destroy the malaria, and if not certain, will the expense not be a most alarming one? Can any one venture then on giving advice on the subject? This last is the very point which I look on as so clear.

* See "*Is the Eucalyptus a fever-destroying tree?*" by I. Bosisto. Roy. Soc. of Victoria, 1874.

I do not pretend to give from my own experience any information on the curative powers of the oil, but I have read enough on the subject to satisfy me that the character given to the plant is well founded. A brochure by Dr. Carlotti*, of Corsica, goes over the whole subject very fully, and journals in abundance have treated the subject. But strong as I believe the arguments are, I would conclude that even if they were weak the tree ought to be encouraged and grown largely, because if the benefit of the curative agency were less there is another advantage arising, and that is from the nature of the wood. I went to Tre Fontane with a friend who planted about a million and a half trees in this country annually, and had studied the growth carefully, and he said that the best growing tree would require about thirty years to grow the wood made in five years by the eucalyptus. The question I then asked was this: Does this rapid growth arise from the difference of climate in Scotland and Italy? But the surprise of the Italians at the rapid growth was equally great, although I am not able to tell the difference of increase of the same kind of tree in the two countries. See on this also the book quoted.

M. Lambert, quoted by M. Carlotti, gives the growth in Algeria thus:—

Circumference in centimetres after			1 year	...	10
„	„	„	2 years	...	13
„	„	„	3 „	...	30
„	„	„	4 „	...	40
„	„	„	5 „	...	55
„	„	„	6 „	...	75
„	„	„	7 „	...	90
„	„	„	8 „	...	1·20
„	„	„	9 „	...	1·50

M. Carlotti has seen the growth from 11 months to be equal to 17 cent. in circumference.

* Assainissement des Régions chaudes insalubres par Regulus Carlotti. (Ajaccio, 1876).

Wood about Rome is scarce; people are cold in winter and would gladly use a fire, and they put cloaks on even in their houses I am told. True, their winter is short. It is a very expensive thing to have a fire in a hotel, and one can easily burn five francs' worth of wood in a day, or at the rate of seventy pounds' worth a year for a fire in one small room. People have not fires enough for the wants of a full civilisation. Even large hotels allow their fires to go out in the evening, and cooking at unusual times cannot be accomplished. The wood of a gum tree five years old is larger than necessary for firewood, and would at any rate be of a most convenient size for splitting up. It is too large near the ground to be used entire, and much larger than any of the wood which I got in Rome or Florence.

Let us imagine this as a source of fuel, a portion of the Campagna, Maremma, Pontine marshes, &c., growing such a crop of combustible matter every five years. Where else will be found a coal field equal in value? Let the power obtainable from this be calculated.

Professor Boyd Dawkins tells me that Sir Charles Nicholson, late Governor of Queensland, had proposed planting the gum trees in Italy twenty years ago, and had sent seed enough to have covered the Maremma with trees, but we see what has been done.

Speaking with a Roman senator on the subject, he said it would take thirty years to grow the trees, and man was a shadow and could not look so far. I know that man is mortal, but have often heard that Rome is eternal, and it is for the future we must act. This senator had the same idea we have here of thirty years for a pretty strong stem, but now it is reduced to five with apparent certainty. These five years themselves may be reduced, because at two years the trees give out rich odour, and perhaps when still younger, and thus they begin to work almost at once, preparing wholesome habitations. The evidence seems to be

that in the second year the homesteads would with care be habitable, and in five years there would be a rich crop of wood off the plantation, whilst the ground cultivated for other purposes would be richer of course, because it would be receiving that attention which for a long time it never has had. The trial in this point of view ceases to be a speculation; it is a fine opening for producing wealth and comfort. Even if the malaria were not removed the gain from the wood would be great.

It is said that the tree will not grow if the temperature falls below 4° C, or about 25°—26° Fahr. One specimen had lost the top leaves by frost; but the great fact remains that they lasted the five years required. There are many varieties of the tree, and some grow in Australia at the height of 4,000 feet, but I cannot learn if these produce as much wood or oil as the globulus; if they do, they promise to be valuable in this country. The eucalyptus viminalis was said to be the best for drying up swamps, and it had the appearance of a willow; but it was by no means so full of oil to all appearance or so fragrant as the eucalyptus globulus. The eucalyptus coccifera never suffered at all from the frost at Tre Fontane, but it had not been long tried; still the attempt was made in its tenderest years.

M. Bosisto says that the Eucalyptus Amygdalina grows 350 feet on high undulating forest land, but not above 100 miles from the coast. This may be the best species for Italy. M. Carlotti seems to recommend the Globulus; M. Bosisto's remarks seem to favour the amygdalina.

The conclusion arrived at was that the frost of the Campagna would not hurt to any great extent any species of the tree tried there, and the principal specimen especially. Nearer the Sabine hills it was said not to grow so well, but there is abundant low land where it is required.

The value claimed for the eucalyptus is intelligible if we consider it as continually giving out a medical agent. So

far as we know this medical agent, when constantly inhaled, has no unfavourable effect, a result not very usual; but it is difficult to compare it with others, because in no cases can we take medicine in doses so minute, constant, and equable. There is room here for a very interesting inquiry.

One virtue claimed for the tree is not so clear—namely, its drying up of swamps. This has been accounted for by its wonderful rapidity of growth; but I do not hear that it requires wet ground to grow upon, and am told that it grows in Australia in very dry places.

M. Bosisto mentions cool refreshing draughts of water obtained from the trunk of the *dwarf trees* in the region of the river Murray. Still one would be glad to hear more on this point. These dwarfs are only 25 feet high.

A paper read before the Royal Society of Victoria by I. Bosisto shews a great variation in the amount of oil. He says the *Euc. Odorata* yields 7 fluid ounces from 2000 lbs. of leaves attached to small branchlets.

Viminalis...............	the same.
Rostrata	15 ounces.
Obliqua	80.
Globulus	120.
Sideroxylon............	160.
Oleosa	200.
Amygdalina............	500.

I suppose these numbers to represent the total amount of oils and resins.

In a report on the strength of wood, published by the Science and Art Department, Kensington, the Eucalyptus stands high, but as there are several species from several situations the numbers vary considerably.

In a list where the highest breaking weight is 11,158 lbs. in the case of white or pale iron bark, the blue gum of N.S. Wales is 7,364 to 6,860, of Queensland 5,663, 4,074, 3,416, and as a means of comparison, may be given Russian

Larch 2,142, White Cedar, Queensland, 2,105; the blue gum, therefore, takes a high position.

That drying is an important element in the removal of the causes of malaria cannot be doubted in the most of cases, but it is by no means apparent in all. It cannot be doubted that Garibaldi's plan of having another passage for the Tiber south of Rome would be of great advantage, but especially to the city itself, which is so frequently inundated, and which must, therefore, be on a foundation exceptionally wet. The mud brought down is proverbially great; but so far as I could judge it was not so fine as that of the Nile, which, however, I have seen only in bottles. The Egyptian mud causes no fevers; perhaps because it is more rapidly dried up. The mud of the Tiber is deposited on a very deep soil, but the same occurs on the Nile. The depth of mud, however, may have an important bearing in a less burning sun.

The mud of the Tiber seems always to have been remarkable, and one wonders why it should be able to go on so long unchanged, but on going up the river the difficulty ceases; there we find mud hills, many of them conical, all with steep sides and nearly all without any vegetation except below in some cases. There is in fact from water an enormous deposit raised to a great depth, and enough to keep the Tiber as muddy as ever for ages. It seemed to me that all this fine earth would be very valuable in much of our country, where we are glad of a few inches over chalk, or of a little soil scattered among large stones. Here it lies in great excess, cut into millions of little valleys, a fine study for those who wish to examine the action of streams, but a land wasted and almost useless. If these were made to bring out crops there is room enough for a great population to live, and if crops were closely grown, the mud would proportionately cease to flow. There is a good deal of friendly interchange of favours to be made between the lower and upper Tiber. That this is possible seems to be proved from

some of these hills being covered, although there are few in this condition. It may be difficult to coat the steep sides with any vegetation, but when the top is covered this difficulty diminishes. Still this is work for many men and for a long time.

A peculiar feature of the unwholesome land struck me. It began exactly at the foot of the hills. There was no gradual slope, but you came down from the hill and were at once on the plain stretching out for miles. This, of course, favours violent floods, and may be coexistent with very bad drainage of the plain. Still, as it has been remarked, the Campagna around Rome is not a plain surface, neither are the lower parts the least healthy.

I hope to think more fully of this if I describe the various opinions on the cause of malaria; at present it comes only incidentally. The great engineering works which only governments can undertake promise to be of value, but the planting to which I specially allude seems to point it out as the work of private men. This view of the subject, if correct, must be most comforting to Italy, because it prevents that delay which is needful before great numbers can be brought to act in concert. If enough can be done privately to enable the agricultural labourers to inhabit the plains without fear, the larger schemes will come in time. The Tiber is dammed up by bridges, and one, namely Pons Sublicius, lies now at the bottom, and the water flows over with difficulty. No wonder the bed rises, and no wonder the Tiber bursts all bounds at times. It did so even in the time of Horace, who tells us in his second ode his fear of another flood as in the time of Pyrrha, when the fish rose into the trees or paid a visit to the mountains.

This shows that even by reducing the bed of the Tiber to the level it had in the time of Augustus, floods would not cease, and deepening is not at all likely to be a sufficient measure. It is a pity that the double bed should be so

expensive. That is, however, not for private action; but the use of the eucalyptus is for all who live on the plaius out of town.

This fear is greater now, and the ground less wholesome; but of that and the inquiries of Dr. Balestra and others I hope to speak after some time. At present I shall content myself with a few recommendations.

The conclusion to which I come is that enough is known to prove that great benefit may be expected from the cultivation of the eucalyptus globulus and perhaps more from other species. The best plan to begin would probably be by surrounding with trees any house to be inhabited in a district subject to malaria. That the benefit seems to arise in the second year, as soon as the plants give out a great deal of odour. That the experiment near Rome is very small and somewhat mixed, but is nevertheless enough to give great hope to those who have an opportunity to try the same in the Campagna and elsewhere. That the experiments made in other countries seem to indicate a certainty of benefit. That, the value of wood being great near Rome, the eucalyptus promises a substance much wanted and there is therefore a double reason for growing it. That the growth is unusually rapid, so much so that even in five years the wood is much larger than that usually employed for burning in Rome. The expense of the crop is therefore small, and Rome is thus promised both a supply of health, of warmth, and of power by the outlay of little money and with little labour. Apparently Rome need never envy us our coals, for her coal thus got will last for ever.

I have made no new discovery in relation to this subject, but have only put together a few of the more striking points,

that my Roman friends may be induced to stimulate action in those who are more directly concerned, and not leave neglected such a promising source of comfort and of national wealth. For not only is this great source of power to be obtained in the rapidly growing wood, but the removal of malaria is the introduction of better cultivation and additional wealth from other crops.

Professor BOYD DAWKINS. F.R.S., remarked that in Australia and Tasmania the eucalyptus grows in almost every sort of situation and from the sea level to a height of several thousand feet above the sea. It is to be seen growing equally well on arid rocky soil based on thick sandstones, analagous to our millstone grit, and on swampy ground. In the Blue Mountains a ridge of carboniferous rock rising in New South Wales to a height of 3,000 feet above the sea, it is subjected to considerable extremes of climate. At Mount Victoria, for example, on the 7th of August, 1875, there was a heavy fall of snow followed by a frost; and this, I was told, was by no means unusual in the winter. It seems, therefore, that some varieties of eucalypti can stand a moderate amount of cold, and I see no reason why, if it be thought desirable to grow so ugly a tree, it should not be naturalised in this country. On this point, however, I have a letter from Sir Charles Nicholson, Bart., which is an important contribution to the history of the introduction of the plant into Europe.

[COPY.]

"Some years since I wrote to the Government botanist at Melbourne, suggesting to him the possibility of acclimatising

some of the harder varieties of the eucalyptus in the British Islands; and at the same time asking him to procure me the seed of any plants belonging to that family whose habitat was alpine, or that belonged to the most southern and colder parts of Tasmania. In compliance with this request he sent me a collection of seeds, which, however, I suspect were nearly all derived from the neighbourhood of Melbourne, Gipps Land. I had them sown in pots, and kept the young plants in a greenhouse for a couple of years. They grew with such rapidity that I no longer had any space for them, and after giving away a number of plants, I turned about half a dozen out into the open air. Three or four of the specimens (they were all of the a. eucalyptus globulus or quadrangular) seemed scarcely to feel the change, and in the course of some four years attained a height of about ten or twelve feet. The other specimens put out in the open air were cut down each winter, growing again from the root with the return of each succeeding spring.

"Such has been the result of my experience up to within the last fortnight. Two days ago, however, on going to my farm in Essex, I was disappointed to find that my trees, which had so successfully resisted the frosts of four winters, had in consequence of the late severe weather been seriously affected, the greater part of the foliage and the more tender branches being shrivelled up and dead. I do not imagine, however, that the trees themselves will be destroyed; they will probably lose some two or three feet of their height and again flourish, if they escape a succession of severe winters.

"Were I to attempt—and I regret that I have not done

so—the cultivation of the eucalypti, I would arrange the plants in thick plantations, mixed with pines or nondeciduous trees, in order that the latter might afford protection to the young plants during their earlier growths. I find that the stokea, an Australian genus, grows perfectly well and is wholly unaffected by the winter climate of England: it would prove a good associate with the gum tree.

"Some twenty years since, on landing at Naples, and seeing so many plants of the Australian flora flourishing there, it occurred to me that the cultivation of the gum trees might be most advantageously followed throughout the whole of the Campagna. I tried to impress some of the official people in Rome whom I met with the idea, amongst others, Monsignor Talbot, whom I knew, and he engaged, if provided with the necessary supply of seed, that the experiment should be tried. At the cost of some trouble and expense, I procured a quantity of seed, sufficient by this time to have covered the whole Maremma and Pontine marshes with a forest of gum trees. But I believe the attempt in those good old days of Papal supineness was never made to initiate an experiment which, if successful, would transform the climate of Italy, and be of value in a thousand different ways.

"The coincidence of the presence of the eucalyptus and the infrequency of malaria is a most curious one. The investigation of the cause is one well worthy of pursuit."

In a second letter Sir Charles gives an account of the eucalyptus in India and New Zealand:

"In 1861 I went through India, and on visiting the Nilgherry hills was delighted to see the accacias, eucalypti and

angophorus growing, especially the first (the accacias) in the greatest luxuriance. I afterwards learnt from Sir William Denison (the then Governor of Madras) that many millions of plants had been established and were growing in different parts of the Presidency, and that the eucalyptus had been found to thrive on arid spots and places where no other trees could be grown. As a source of future supply for timber the cultivation of the eucalyptus is of course an object of the very highest importance. As you are probably aware, hundreds of thousands of acres of land that a few years ago in New Zealand were open downs or treeless plains, are now clothed with a thick forest of the indigenous trees (especially the eucalyptus) of Australia."

Annual General Meeting, April 18th, 1876.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Robert Ellis Cunliffe, of Manchester, and Mr. Thomas Hornby Birley, of Somerville, near Manchester, were elected Ordinary Members of the Society.

Report of the Council, April, 1876.

The Treasurer's account shows that in consequence of the large outlay required for the repair and beautifying of the property, the renewal of upholstery, the charges for the incorporation of the Society, binding books, and a new catalogue, the expenditure for the year has been £324 11s. 11d. in excess of the income, and has diminished the balance to that amount; but the ordinary expenditure, allowing an average amount for binding and assistance in the library, would be within the income.

The ordinary balance is £94 8s. 7d., and there is due to the Society one year's rent from the Geological Society, which would make £125, less accounts owing for periodicals, £10, and voted for binding and not paid, £27, leaving an available balance of about £88.

The number of ordinary members on the roll of the Society on the 1st of April, 1875, was 169, and 3 new members have since been elected; the losses are—deaths, 2; resignations, 2; and defaulters, 2. The number on the roll on the 1st of April instant was, therefore, 166. The deceased members are Murray Gladstone and William Jackson Rideout.

Mr. Murray Gladstone, who died on August 23, 1875, was the fifth son of Robert Gladstone of Liverpool, where he

was born on the 14th of February, 1816. He was educated as an Engineer, being articled to the late George Stephenson. He followed this profession for several years, after which he became a member of the firm of Ogilvy Gillander and Co., and went to Calcutta, where he remained for several years. On the 30th April, 1861, soon after his return to England, Mr. Gladstone was elected a member of this Society. He then resided at Higher Broughton, at the house which is now Bishop's Court. He here set up an observatory with a 7¼-inch refracting telescope equatorially mounted. On the removal of the Crumpsall observatory to Altrincham Mr. Gladstone placed his observatory at the disposal of Mr. Baxendell. Some of the results of the observations made at this observatory have been published in the Society's Proceedings. On leaving Manchester he removed his observatory to his new residence, Penmaenmawr.

The Council having received a request from the Commissioners of the Loan Exhibition of Scientific Apparatus to contribute any interesting objects belonging to the Society, have, with the assistance of Dr. Roscoe, selected such of the late Dr. Dalton's apparatus and instruments as appeared to be of the greatest historical interest, and have sent the collection to South Kensington for exhibition.

The following papers and communications have been read at the ordinary and sectional meetings of the Society during the session now ending:—

October 5th, 1875.—"On a Glue Battery," by Dr. J. P. Joule, F.R.S., &c.

"On the Red Marls under Manchester," by E. W. Binney, F.R.S., V.P.

October 11th, 1875.—"On the Hybrid British Heath, Erica Watsoni, Benth.," by Charles Bailey, Esq.

October 12th, 1875.—"On a Source of Atmospheric Ozone," by Joseph Baxendell, F.R.A.S.

October 19*th*, 1875.—"On some Reactions of Bromine and Iodine," by Professor C. Schorlemmer, F.R.S.

"On some Bronze Coins found sixty years ago under a Peat Bog at Miserton Car, in Notts," by E. W. Binney, V.P., F.R.S.

"Notes bearing on Mathematical History," by Sir James Cockle, F.R.S., Corresponding Member of the Society.

"Note on the Temperature of the body during Physical Exertion," by M. M. Pattison Muir, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

November 2*nd*, 1875.—"On a Lead Pipe which had been transformed into Galena," by Peter Spence, F.C.S.

"On the principle of the Electro-Magnet constructed by Mr. John Faulkner," by Professor Osborne Reynolds, M.A.

November 8*th*, 1875.—"The Fauna of Cymmeran Bay, Anglesea, Part II.," by John Plant, F.G.S.

November 16*th*, 1875.—"On an Instrument for Measuring the Direct Heat of the Sun," by Professor Balfour Stewart, LL.D., F.R.S.

"On a Colorimetric Method for Determining Small Quantities of Copper," by Thomas Carnelley, B.Sc., F.C.S. Communicated by Professor H. E. Roscoe, F.R.S., &c., &c.

November 30*th*, 1875.—"On the Estimation of very small quantities of Lead and Copper," by M. M. Pattison Muir, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

"On certain circumstances which affect the Purity of Water supplied for Domestic Purposes," by M. M. Pattison Muir, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

December 14*th*, 1875.—"On a Sample of Peat from lagoons in the Sierra Madre in Mexico," by Professor Schorlemmer, F.R.S.

"On Graphic Methods of Solving Practical Problems," by Professor Osborne Reynolds, M.A.

"On Explosions of Fire Damp," by E. W. Binney, V.P., F.R.S.

"Chemical Notes," by M. M. Pattison Muir, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

December 28*th*, 1875.—"On the Utilization of the Common Kite," by Dr. Joule, F.R.S., V.P.

"On the Migration of Swallows," by E. W. Binney, V.P., F.R.S.

January 11*th*, 1876.—"Note on a Method of Comparing the Tints of Coloured Solutions," by J. Bottomley, D.Sc.

"On Explosions of Fire Damp," By Robert Rawson, Esq., Hon. Member of the Society.

January 17*th*, 1876.—"On the Life History of Lymexylon Navale," by J. Sidebotham, F.R.A.S.

"On Psammodius Sulcicollis'" by J. Sidebotham, F.R.A.S.

January 25*th*, 1876.—"Stannic Arsenate," by William Carlton, Williams, F.C.S.

February 8*th*, 1876.—"On the Natural Gas from the Gas Wells in Butler County, Pennsylvania," by Professor Sadtler, of the University of Pennsylvania. Communicated by Professor C. Schorlemmer, F.R.S.

"Notice of a recent discovery of a pre-Historic Burial Place, near Colombier, in Switzerland," by William E. A. Axon, M.R.S.L., &c.

"On the Granites of Ravenglass and Criffel," by Wm. Brockbank, F.G.S.

"On Boulder Stones in the Manchester Drift," by E. W. Binney, V.P., F.R.S.

"On the Formation of Azurite from Malachite," by Charles A. Burghardt, Ph.D.

"On a Direct-Vision Spectroscope of great Dispersive Power," by Arthur Schuster, Ph.D.

"On a New Absorptiometer," by Arthur Schuster, Ph.D.

February 14*th*, 1876.—On a New British Moss—Hypnum nitidulum (Wahl)," by Mr. James Percival.

"On the Gradual Decrease of Wild Birds during the last 25 years West of Manchester," by John Plant, F.G.S.

February 22*nd*, 1876.—"Notes on a Collection of Apparatus Employed by Dr. Dalton, in his Researches, which is about to be Exhibited (by the Council of the Literary and Philosophical Society of Manchester) at the Loan Exhibition of Scientific Apparatus, at South Kensington," by Professor Roscoe, F.R.S.

"On the Zoological Station and Aquarium at Naples," by Arthur William Waters, F.G.S.

"On Glacial Action in the Valley of the Weir, etc.," by Professor T. S. Aldis, M.A.

February 29*th*, 1876.—"An Account of some early Experiments with Ozone, and Remarks upon its Electrical Origin," by J. B. Dancer, F.R.A.S.

"Results of Rain Gauge Observations, made at Eccles, near Manchester, during the year 1875," by Thomas Mackereth, F.R.A.S., F.M.S.

March 7*th*, 1876.—"On Crystals of Sulphate of Lead found in Alum Residue," by R. S. Dale, B.A.

"On the Degree of Accuracy displayed by Druggists in the Dispensing of Physicians' Prescriptions in different towns throughout England and Scotland," by William Thomson, F.C.S.

March 13*th*, 1876.—"On a Series of Specimens of very young Rhombus Vulgaris (Cuv.)," by R. D. Darbishire, F.G.S.

"Notes made during a Visit in the past Summer to the Swedish Shell-beds of Uddevalla and the neighbouring district," by R. D. Darbishire, F.G.S.

"List of Shells found in Cymmeran Bay, Anglesea. Corrections and Additions," by John Plant, F.G.S.

March 21*st*, 1876.—"On a Graphical Method of Drawing Spectra," by Mr. William Dodgson, Whitworth Scholar. Communicated by Professor Roscoe, F.R.S., &c.

"Evidence to prove that a Bone from the Windy Knoll, Castleton, named by Professor W. Boyd Dawkins, F.R.S., 'Sacrum of young Bison,' is a Sacral Bone of the Cave Bear, Ursus Spelæus," by John Plant, F.G.S.

April 4*th*, 1876.—"On the Depreciation in the value of Silver," by Professor W. Boyd Dawkins, F.R.S.

"On some Isomerides of Alizarine," by Edward Schunck, Ph.D., F.R.S., and Hermann Roemer, Ph.D.

"Note on Mr. Plant's Fossil Sacrum from Windy Knoll," by Professor W. Boyd Dawkins, F.R.S.

"The Eucalyptus and Malaria, by Dr. R. Angus Smith, F.R.S., V.P.

"On the Eucalyptus," by Professor W. Boyd Dawkins, F.R.S.

Several of the above papers have been passed by the Council for printing in the Society's Memoirs. The printing of Vol. V., third Series, of the Memoirs has been completed, and the copies are now in the hands of the bookbinders.

The Council again consider it desirable to continue the system of electing Sectional Associates, and a resolution on the subject will be submitted at the annual meeting for the approval of the members.

The Librarian reports that since the last annual meeting a large number of volumes have been bound and the catalogue has been printed. The number of societies with which we correspond continues nearly the same as last year. Copies of the catalogue can be had by members on application.

On the motion of Dr. BORGHARDT, seconded by Mr. R. S. DALE, the Report was unanimously adopted, and ordered to be printed in the Society's Proceedings.

On the motion of Mr. BAXENDELL, seconded by Mr. BAILEY, it was resolved unanimously:—

That the system of electing Sectional Associates be continued during the ensuing Session.

The following gentlemen were elected officers of the Society and members of the Council for the ensuing year:

President.

EDWARD WILLIAM BINNEY, F.R.S., F.G.S.

Vice-Presidents.

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.
JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., F.C.S.
ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.
HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S., F.C.S.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.
OSBORNE REYNOLDS, M.A.

Treasurer.

SAMUEL BROUGHTON.

Librarian.

FRANCIS NICHOLSON, F.Z.S.

Of the Council.

REV. WILLIAM GASKELL, M.A.
ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
WILLIAM BOYD DAWKINS, M.A., F.R.S.
BALFOUR STEWART, LL.D., F.R.S.
CHARLES BAILEY.
CARL SCHORLEMMER, F.R.S.

On the motion of Mr. HENRY H. HOWORTH, seconded by Mr. JOHN A. BENNION, it was resolved:

That the Council be requested to consider the amendment of the present imperfect system of election of officers of the Society.

On the motion of Mr. SPENCE, seconded by Dr. R. ANGUS SMITH, it was resolved unanimously:

That a Memorial be presented to the Mayor and Corporation of the city of Manchester expressing the regret of the Society to hear that it is proposed to transfer the Reference Library to the upper rooms of the new Town Hall, and urging the importance, in the interests of science and literature in Manchester, of having the valuable books of reference belonging to the city placed in a central locality in rooms which will afford the easiest possible access and large facilities for study.

THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Balance Sheet from April 1st, 1875, to March 31st, 1876.

SAMUEL BROUGHTON, TREASURER.

Dr.

		£ s. d.	£ s. d.	£ s. d.
1875 April 1.—To I	…………			542 1 1
"	…es, 1874-5…………	4 4 0		
	11 " Subscriptions " …………	23 2 0		
			27 6 0	
	169 Members on Roll, at £2 2s. …………	354 18 0		
	Less Compounders 4 } " in Arrear 20 } " …………	50 8 0		
			304 10 0	
	2 Admission Fees, 1875-6 " …………	6 6 0		
	3 Subscriptions elected, 1875-6, at £2 2s.……	6 6 0		
			12 12 0	
	2 Associates, Microscopical Section ……		1 0 0	
			345 8 0	
"	…………		2 6 7	
"	…………	2 2 0		
	…………	2 2 0		
			4 4 0	
	…use of Room, 1874-5 ….	30 0 0		
	" 1874-5 ….	34 10 0		
	…Female Education …	13 15 0		
			78 5 0	
	Bankers' Interest…………		8 16 0	
				438 19 7
"	Interest from the Owens College on £1500 Natural History Fund to December 25, 1875, less tax at 2d.……			59 10 0
				£1040 10 8

General Balance:

	£ s. d.	£ s. d.
Compound Fund …………		98 15 0
Natural History Fund, last year…. 24 5 7		
" " 1 yrs. Interest 59 10 0		
	£83 15 7	
Voted for Books …………	40 0 0	
		43 15 7
Other Balance …………		94 8 7
		£236 19 2

Cr.

		£ s. d.	£ s. d.
1876. March 31.—By *Charges on Property*:			
	Chief Rent …………	12 16 2	
	Insurance …………	7 0 0	
	Property Tax …………	5 12 4	
	Painting…………	33 8 8	
	Gasfitting …………	21 0 0	
	Repair of Roof, Re-flagging, and other Repairs ..	66 16 11½	
			146 14 1½
"	…………	2 2 6	
	…………	10 11 2	
	…………	6 12 8	
	…………	18 6 4½	
	…………	54 9 3	
			92 3 6½
"	…………	57 4 0	
	…………	21 19 0	
	…………	18 10 1	
	…………	12 16 0	
			110 9 1
"	Law Charges for Incorporation of Society …………		31 6 10
"	*Publishing*:		
	Memoirs and Engraving …………	50 0 6	
	Proceedings…………	33 1 0	
	Editor of Memoirs and Proceedings …………	50 0 0	
			133 1 6
"	…………	16 17 9	
	…………	73 9 2	
	…………	3 5 0	
	…………	31 10 0	
	…logue ..	67 14 0	
	…, 3 years	2 2 0	
			194 17 11
	Total Expenditure…………		763 11 6
"	Natural History Section for Purchase of Books…………		40 0 0
"	Balance …………		236 19 2
			£1040 10 8

Examined and found correct,
April 5, 1876.

W. A. CUNNINGHAM.
WM. BROCKBANK.

Ordinary Meeting, April 18th, 1876.

EDWARD SCHUNCK, Ph.D., F.R.S., President, in the Chair.

"Note on a Church Bell, at North Wooton, Somersetshire, dated A.D. 1265, in Arabic Numerals, and on a MS. dated A.D. 1276, in which they are freely used," by WILLIAM E. A. AXON, M.R.S.L., &c.

The date at which Arabic numerals were introduced into this country has been a matter of considerable doubt and discussion. There are very few to be found upon our monuments and public buildings earlier than the 16th century. Dates have frequently been cited, such as 1090, 1102, and so forth, but closer examination has proved that they have been misread. There is a doubtful date which may be 1417 given in the Archæological Journal, Vol. VI, p. 291. Heathfield church contains the date 1445, and Lych-gate, at Bray. Berkshire, is dated 1448. In documents the Arabic numerals are only occasionally met with in England in the fifteenth century, and only two examples are known belonging to the fourteenth century. Mabillon, after examining six thousand European MSS., found no earlier instance of the use of these figures than one of 1355, in the handwriting of Petrarch. Having written an article on the history of the Arabic numerals in the *Companion to the Almanac* for 1875, in which these dates were given, I received from Mr F. W. Dunkerton, an interesting rubbing of a bell inscription, which I now exhibit. The bell is in the church of North Wootton, near Wells, in Somersetshire,

The church is about seven miles from Glastonbury, and there is a local tradition that one of the bells once belonged to the famous abbey of that place. Mr. Dunkerton's curiosity being thus excited, he examined the bells. Two are modern, but the largest is an old one, and round it towards the top is this inscription :

ANNO DOMINNI AP. W.L. CW. 1265.

The letters appear to be cast, and not incised.

If we were to accept this date as accurate, it would upset all our previous notions upon the subject. There are other reasons for regarding it with doubt. The earliest known English church bell is that at Claughton, near Lancaster, which is dated ANNO DOMI M.CC. NONOG. VI. (1296). The North Wootton date is thirty-one years earlier. It would be strange if the oldest bell should chance to be also the earliest example of the use of the Arabic numerals in English inscriptions. Dates are but rarely found on mediæval bells. The earliest instance of the use of Arabic numerals on them with which I am acquainted is at Eglingham church, near Alnwick. The inscription is in German, and the date 1489. Strasburg is said to have one bell dated 1461, and another 1474. There is therefore strong probability against the accuracy of this North Wootton date. The style of the letters remind one of a later period. Apart from the date, most persons would be inclined to attribute them at the latest to the 16th rather than the 13th century. The three sets of initials seem also to indicate a comparatively recent age. Mr. H. T. Ellacombe, in his work on the "Church Bells of Somersetshire," suggests that the date should be read 1625, the figures being out of order. This seems impro-

bable. Mr. Ellacombe does not appear to have seen the bell itself. The letters have a strong resemblance to those used by Roger Semson, of Ash Priors, who was at work in the middle of the 16th century.

The vine leaf ornament which is observable over the inscription suggests an even later date. It was a form of decoration frequently used by a family of bellfounders at Closworth, one member of which, Thomas Purdue, died in 1711, aged 90 years. The letters he used strongly resemble those employed by Semson.

Mr. J. A. Picton, F.S.A., who has paid a good deal of attention to the history of numerals, writes to me that he is inclined to identify the second figure with the gobar figure 5, though it differs in position, and the last figure with the gobar 4. In this case the date would be 1564. Whilst agreeing with him in assigning the inscription to the 16th or 17th century, I am unable to solve the riddle of the date in a satisfactory manner. It is certainly the most puzzling of all those which seem to carry back the use of Arabic numerals to a much remoter date than is commonly allowed. On this general subject, however, the last word has not yet been said. Since the publication of Mr. Picton's memoir "On the Origin and History of the Numerals" in the *Transactions of the Liverpool Literary and Philosophical Society*, 1874, and of my own paper in the *Companion to the Almanac*, 1875, we have both heard of a MS. of the 13th century, in which the Arabic figures are freely used. It is a treatise on the Astrolabe, by Macha-allah, or Messahala, and is dated 1276. This is now one of the treasures of the Cambridge University library, where it is marked Ii. 3.3.

A portion of it has been printed in the Rev. W. W. Skeat's edition of Chaucer on the Astrolabe (*Early English Text Society*, 1872.)

This shows that the Arabic numerals were in use at a date much earlier than is generally supposed. A critical examination of this MS. and of the form of the numerals employed would probably throw more light upon this interesting subject.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 28th, 1876.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

ARTHUR SCHUSTER, Ph.D., and R. F. GWYTHER, B.A., were elected members of the Section.

The following gentlemen were elected officers of the Section for the ensuing year:—

President.
E. W. BINNEY, F.R.S., F.G.S.

Vice-Presidents.
JOSEPH BAXENDELL, F.R.A.S. | J. B. DANCER, F.R.A.S.

Treasurer.
SAMUEL BROUGHTON, ESQ.

Secretary.
THOMAS MACKERETH, F.R.A.S., F.M.S.

April 25th, 1876.

ALFRED BROTHERS, F.R.A.S., in the Chair.

Mr. BAXENDELL read a paper "On the Connexion between the Humidity of the Air and the Amount of Ozone," in which it was shown from observations made at the Southport Observatory during the four years ending June 30th, 1875, that in the months of January, February, March, September, October, November, and December the amount of ozone was above the mean when the relative humidity of the air was below the mean, while in the months of April, May, June, July, and August it was above the mean when the relative humidity was also above the mean. The excess with a relative humidity below the mean was greatest in the months of September, November, and December; and

with a relative humidity above the mean the greatest excess of ozone occurred in April and May. The results of the four years' observations are as follows:—

	Mean Amount of Ozone, with the Relative Humidity of the Air		
	Below the Mean.	Above the Mean.	Difference.
January	5·6	4·5	+1.1
February	4·8	3·9	+0·9
March	5·4	5·1	+0·3
April	4·0	5·7	—1·7
May	5·2	6·8	—1·6
June	5·2	6·2	—1·0
July	5·3	5·4	—0·1
August	4·9	5·1	—0·2
September	6·4	4·1	+2·3
October	5·0	3·7	+1·3
November	5·8	3·5	+2·3
December	5·8	3·7	+2·1

MICROSCOPICAL AND NATURAL HISTORY SECTION.

April 10th, 1876.

A. Brothers, F.R.A.S., in the Chair.

Mr. Sidebotham, F.R.A.S., made the following remarks.

On the discovery of Ægialia rufa.

In the middle of last June I captured two specimens of a species of Coleoptera new to me. This proves to be *Ægialia rufa* of Fabricius, a species found in North Germany, but said to be rare. *Rufa* differs from the other two British species in being of a red colour, the prothorax is rugose and margined at the base, and the elytra strongly crenate striate. The specimens found were on the loose sand at the base of the sand hills at Southport. I send one of the specimens for exhibition, also specimens of the other two allied species.

Mr. T. Rogers communicated the following:—

I recorded the discovery of a *Jungermannia* which has been provisionally named *Jungermannia Nevensis*, by Dr.

Carrington. It was discovered by Mr. John Whitehead during a botanical excursion to Ben Nevis, July, 1875. It has not yet been described, but is near in form to *Jungermannia Starkii*, and probably may have been overlooked for that species. Dr. Carrington thinks it may be new to science, but he confirms Mr. Whitehead's surmise that it is new to Britain, and it will consequently appear in the appendix to Dr. Carrington's work on British Hepaticæ. It grows in moderate sized patches on the sloping banks of the mountain. Neither male nor female flowers have yet been found.

Professor W. BOYD DAWKINS, F.R.S., exhibited a series of specimens which he obtained in September, 1875, from the inner side of the barrier reef at Honolulu, together with photographs. At that point of examination the reef was formed of irregular nodular limestones composed in part of corals, millepores, and perfect shells, but mainly of fragments so broken up and altered by the action of carbonic acid that the original structure was almost lost. The fine calcareous sediment forming the bottom of the lagoon, and covering the shore up to the very edge of the lofty palms was analogous in every respect to that which composes the oolitic limestones, and especially the Bath or great oolite. In the deposits on the inner side of the reef, as well as in the limestones above mentioned, corals were equally distributed. The masses of coralline limestone were traversed by Serpulæ, just as in the case of the Coral Crag.

The few shells which he happened to find were as follows :—

		Range in British Museum.
1. *Triton Chlorostoma*, Lam.	...	West Indies.
2. *Fasciolaria stigmataria*, A.Ad.	...	Sandwich Isles.
3. *Purpura haustrum*, Martyn.		
4. *Nassa hirta*, Hil.	...	Seychelles.
5. *Obeliscus teres* A.Ad.	...	Penang.

			Range in British Museum.
6.	*Cypræa caput-serpentis*	...	New Zealand.
7.	*Cypræa gangrenosa?* Sol.	...	Mozambique.
8.	*Trochus anthosa* (?) *tuberculata?* Gray.	...	New Zealand.
9.	*Natica Brodripiana?* Reclus	...	West Columbia.
10.	*Turbo margaritacea*, Lam.	...	Seychelles.
11.	*Tellina rugosa*	...	New Caledonia.
12.	*Myrtea scalia*, Lam.	...	St. Thomas.
13.	*Terna Californica?* Young. *Terna Cumingii?* Reeve.		
14.	*Modiola tenuistriata*, Dun.		

It is worthy of note that the *Obeliscus teres* of the above list is the nearest living representative of the oolitic genus *Nerinæa* so abundant in the limestones of that formation.

The following communication from Mr. Plant, F.G.S., was read :—

A Beetle of Good Omen from Yucatan.

This curious beetle is found in the Province of Merida, and the Indians find it in the caverns and underground chambers in the ruins of the ancient buildings, described by by Mr. J. L. Stephens in his interesting work on Yucatan. The natives have a strong superstitious regard for these beetles, considering them as powerful charms against evil spirits, and sure to bring good luck to their possessors, so they keep them alive in their houses, and carry them about on their persons. In order to secure the beetle from wandering away, a light band of gold is fastened round the thorax, having a gold chain and hook attached to it; this hook serves to secure the beetle to the curtains of a bed, as a protection during the night from ghosts and evil spirits and also to fasten in front of the dress in the daytime, to wear like a living brooch. The Indians have a belief that the beetle will live for years without food or water, and

that it is quite harmless, having no power to bite, and cannot fly.

It is probable that in a natural state the beetle will live several years, as it belongs to a tribe of coleopterous insects of sluggish habits, concealing themselves in dark and damp places and shunning the daylight.

The whole of this tribe of beetles (Melasoma) is usually uninteresting to coleopterists, being nearly all of dull black, brown, or greyish colours, ugly in forms, and of bad repute for their habits and odours. This particular beetle is oblong, flattish, an inch and half long by half an inch broad (looking much like a large Elater), the thorax and elytra ashy white, a black cross on the thorax, the scutellum and tips of the elytra black, with raised oblong black spots, sparingly on the elytra which are united together underneath, black with numerous ashy pits.

The beetle is Heteromerous, coming under the family of Melasoma *(Latreille)*, in which the British *Blaps Mortisaga* is included, but I have not at present been able to find any authority for the genus and species.

It is an interesting instance of reverence and superstition attached to an insect, and coming from Yucatan, may be one that has descended from the ancient people who built those marvellous cities and temples.

Annual Meeting, May 1st, 1876.

John Barrow, Esq., in the Chair.

Mr. Thomas Hornby Birley, of Somerville, Pendleton, was elected a member of the Section.

The Treasurer's account, and report of the Council were read and passed.

THE MICROSCOPICAL AND NATURAL HISTORY SECTION OF THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY IN ACCOUNT WITH THE TREASURER, H. A. HURST.

Dr.

1876.	£	s.	d.
To Murray's Manuals	1	8	0
„ Archives du Muséum	24	0	0
„ Nouvelles Archives	14	8	0
„ Ditto ditto	4	0	0
„ Bulletin de la Soc. Bot. de France	18	14	0
„ Adams on the Microscope	0	12	0
„ Sectional Subscription	2	2	0
„ Reaumur's Memoires des Insects	2	2	0
„ Ralf's Desmidieæ	3	13	6
„ Subscription for "Ibis" for 1876	1	1	0
„ Teas and Postages	4	8	7
„ Balance in Bank	91	3	4
	£167	12	5

Cr.

1875:				£	s.	d.
May 1.—By Balance at Bank				104	19	8
1876.—Subscriptions:						
One Arrear	£0	10	0			
Seven Associates	3	10	0			
Thirty-four Members	17	0	0			
				21	0	0
Received on account of interest on Grant from late Natural History Society				40	0	0
Interest allowed by Bank to Dec., 1875				1	12	9
				£167	12	5

Examined and found correct,

(Signed) EDWD. W. BINNEY, JNO. DALE, } AUDITORS.

H. A. HURST:

1876.

May. By Balance at Manchester and Salford Bank, St. Ann's-street £91 3 4

The election of Officers for the ensuing year took place as follows:—

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